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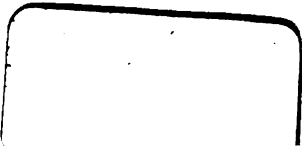
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THE  
JOURNAL OF ANATOMY AND PHYSIOLOGY  
NORMAL AND PATHOLOGICAL.

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CONDUCTED BY

G. M. HUMPHRY, M.D., F.R.S.,  
PROFESSOR OF ANATOMY IN THE UNIVERSITY OF CAMBRIDGE;

W. TURNER, M.B., F.R.S.,  
PROFESSOR OF ANATOMY IN THE UNIVERSITY OF EDINBURGH;

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PROFESSOR OF THE INSTITUTES OF MEDICINE IN THE UNIVERSITY OF GLASGOW.

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# Journal of Anatomy and Physiology.

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THE RELATION OF NERVE-SUPPLY TO MUSCLE-HOMOLOGY. BY D. J. CUNNINGHAM, M.D., *Senior Demonstrator of Anatomy in the University of Edinburgh.*<sup>1</sup>

THE object of this paper is to test the value of "nerve-supply" as a guide in our endeavours to trace the history of a muscle or group of muscles.

The late Professor Rolleston, in his well-known observations upon the serial homologies of the muscles connected with the pelvic and shoulder girdles,<sup>2</sup> employed this means with marked success, but in the determination of ordinary homologies it has been the general custom in this country to rely solely upon a consideration of the (1) position, (2) origin, and (3) insertion of a muscle. In myological memoirs it is rare indeed to find a detailed account of the innervation.

But the *position*, *origin*, and *insertion* of muscles are features which are subject to much variation.

The *insertion*, inasmuch as it involves the action or function of a muscle to a greater extent than the *origin*, is therefore of proportionately greater importance. Many instances, however, might be given to show that it is liable to alteration. I will content myself by quoting a few facts from observations upon the *dorsal interossei* of the foot. As a general rule, these muscles in mammalia are inserted so as to operate as abductors

<sup>1</sup> This paper is an abstract of portions of the report which the author is preparing upon the Anatomy of the Marsupials brought home by the "Challenger" Expedition. It is published by the permission of the Lords Commissioners of the Treasury, and it was read before the International Medical Congress in London, August 1881.

<sup>2</sup> On the Homologies of certain of the Muscles connected with the Shoulder-joint.—*Linnean Transactions*, vol. xxvi.

of the digits from a line drawn through the medius. In man, however, and probably also in the gorilla,<sup>1</sup> they are inserted with reference to the index, and in the lemurs they are inserted with reference to the annular digit. Again, we find in some marsupial and monotrematous feet that certain of the dorsal interossei assume a new and opposite function, viz., approximation of the digits. This they effect by a splitting of their tendon into two parts, which are inserted into the contiguous bases of adjacent toes. I might give many other examples, even from this group of muscles, but those I have mentioned are sufficient to prove that the insertion of a muscle is capable of considerable variation.

The *origin* and, in a corresponding measure, the *position* of a muscle are even more liable to change, and therefore are features upon which we cannot place much reliance. What better example of this could we have than the case of the extensor brevis digitorum pedis? The history of this muscle has recently been very thoroughly and very beautifully worked out by Dr Georg Ruge.<sup>2</sup> It belongs originally to the peroneal group of muscles, and in the monotremata it may be observed arising entirely from the fibula. Its progress as it passes down to the dorsum of the foot can be traced step by step through the mammalian series. First one belly descends, then another, and so on until the entire muscle gains the dorsum of the foot as in man.

We now come to the relation which *nerve-supply* bears to *muscle-homology*. Recently Dr Ruge<sup>3</sup> has enunciated the doctrine that there is an invariable and immutable relationship between *innervation* and *homology*. He asserts that a muscle is to be regarded as the end-organ of a nerve, and therefore, when a muscle alters in position and connections, its original and typical relations can always be identified by its nerve of supply.

Let us test the accuracy of this statement. The most

<sup>1</sup> Bischoff, *Beiträge zur Anatomie des Gorilla*, München, 1879.

<sup>2</sup> A Research into the group of Extensors on the Leg and Foot of Mammalia.—*Morph. Jahr.* 1880.

<sup>3</sup> Processes in the Development of the Muscles of the Human Foot.—*Morph. Jahr.* 1878, p. 187.



convenient method of doing this is to choose one or more nerves and examine these in their relation to the group of muscles they supply in a large number of different animals. It seems to me that the most advantageous nerves to select are the plantar nerves of the foot, and it is convenient to examine them in their connections with the intrinsic group of muscles. Under the term "intrinsic," as applied to the muscles of the foot, we do not include (1) the extensor brevis digitorum, (2) the lumbricales, (3) the flexor brevis digitorum, or (4) the accessorius. Taking, therefore, any digit, we may reasonably conclude that the intrinsic muscles of that digit are homologous with the intrinsic muscles of the corresponding toe in all animals.

The question now is—Are these muscles invariably supplied by the same plantar nerve? I have examined a large number of feet of animals belonging to different orders and genera of mammalia, and find that in the majority the innervation of the intrinsic muscles is identical. It is the same as that which is found in the human foot. The *internal plantar nerve* supplies two muscles of the great toe, viz., the abductor hallucis and flexor brevis hallucis. The *external plantar nerve* is distributed to all the other intrinsic muscles of the foot, i.e., to the adductor hallucis and transversalis pedis (the two remaining muscles of the great toe), and to the intrinsic muscles of the index, medius, annularis and minimus.

In the manus the nerve arrangements are similar to those in the pes. The *median nerve*, which is the representative of the *internal plantar*, supplies only those muscles which are homologous with the flexor brevis hallucis and abductor hallucis of the foot; whilst the *ulnar nerve*, which is the representative of the *external plantar*, supplies all the remaining intrinsic muscles of the hand.

Whilst this is the general rule, and may almost be regarded as the typical distribution of the plantar nerves, several notable exceptions exist, and it is to these I will now direct attention. They are to be found in the feet of the elephant, hyrax, beaver, and foxbat.

The first three of these animals afford examples of an encroachment of the *internal plantar* upon the territory usually occupied by the *external plantar*. The foxbat is an instance of what

seems to be a much rarer form of deviation, viz., an encroachment of the *external plantar* into the domain of the *internal plantar*.

ELEPHANT.—The *elephant* which I had an opportunity of examining was a foetal specimen of *Elephas indicus*, the same indeed as that which is described and figured by Professor Turner in the *Journal of Anatomy and Physiology*, July 1881. The hallux is destitute of intrinsic muscles. A muscular twig nevertheless proceeds from the digital branch of the internal plantar nerve which goes to supply the contiguous sides of the index and medius. It is a twig of some size, and sinking into the sole, it is distributed to both heads of the flexor brevis indicis, and communicates with one of the deep branches of the external plantar nerve.

The intrinsic muscles of the minimus, annularis and medius, and the transversus indicis are supplied by the external plantar nerve.

HYRAX.—In the foot of the *hyrax* the hallux and minimus are absent. As in the case of the elephant, the internal plantar nerve gives off a deep branch which dips into the sole between the flexor tendons for the medius and index, and is distributed to all the intrinsic muscles of the index, and one muscle belonging to the medius, viz., the second dorsal interosseus. The *external plantar nerve* sends off a very minute deep division which supplies the intrinsic muscles of the annular digit.

Although I made a dissection of both feet of the specimen, I was unable to make out with precision the nerve-supply of the flexor brevis medii.

In the hand of the same animal I was much interested to find very much the same peculiarities in the nerve arrangements. The *median nerve* supplies the muscles of the pollex and index and ends in the outer head of the flexor brevis medii. The *ulnar nerve* gives twigs to the muscles of the minimus and annularis.

The second dorsal interosseus muscle which is supplied by the internal plantar in the foot is absent in the hand. The fact of the outer head of the flexor brevis medii of the hand being supplied by the median may simply indicate that it contains in its midst the fibres of this lost interosseous muscle. In the

manus therefore, no less than in the foot, I am doubtful as to the nerve-supply of the short flexor of the middle digit.

BEAVER.—In the *beaver* a still more remarkable deviation is found. The *internal plantar nerve* divides into the usual four digital branches, viz. (1) for the inner side of the hallux, (2) for the adjacent sides of the hallux and index, (3) for the contiguous margins of the index and medius, (4) for the adjoining sides of the medius and annularis. From these, *three muscular branches* proceed for the supply of the intrinsic muscles.

The *first muscular twig* comes from the digital nerve to the inner side of the great toe, and supplies the abductor hallucis. The *second* is the largest of the three, and proceeds from the third digital nerve. It sinks into the sole in the interval between the long flexor tendons which go to the medius and index, and breaks up into filaments which spread out to supply the *adductor hallucis*, the *first dorsal interosseus*, the *flexor brevis indicis* and the inner head of the *flexor brevis medii*. The *third muscular branch* springs from the fourth digital nerve, and dips into the sole between the flexor tendons and the medius and annularis. It is destined for the supply of the *flexor brevis medii*—to both heads of which it gives filaments<sup>1</sup>—and the *third dorsal interosseus*. It communicates on the one hand with the second muscular branch of the internal plantar, and on the other with the terminal filament of the *deep division* of the *external plantar nerve*.

The *deep division* of the *external plantar* is a very small twig, and ends in the *flexor brevis annularis*. The *abductor minimi digiti* is supplied by the *superficial division* of this nerve.

FOXBAT.—The *foxbat* is the only example which I have met of the external plantar nerve extending beyond the usual limits of its muscular distribution and invading the territory of the internal plantar nerve. The *deep division* of the external plantar ends in the substance of the outer head of the *flexor brevis hallucis*. The abductor hallucis is absent in this foot; it follows therefore that the only intrinsic pedal muscle supplied by the

<sup>1</sup> The inner head of the flexor brevis medii has thus a double nerve-supply. With this we must connect the fact that the second dorsal interosseous muscle is absent. It seems to prove that the lost muscle has been absorbed by the inner head of the flexor brevis medii.

internal plantar in the foxbat is the inner head of the flexor brevis hallucis.

If it were allowable to deduce any generalisation from a review of the foregoing examples, we might infer that at some period the internal plantar nerve was as much, or perhaps more, concerned with the supply of the intrinsic pedal muscles than the external plantar nerve. Judging from the foxbat there now appears to be a tendency for the external plantar (in so far as this group of muscles is concerned) to drive the internal plantar out of the field altogether. I am quite aware that in the case of the foxbat it may be objected that the muscle in which the deep division of the external plantar nerve ends is not the outer head of the flexor brevis hallucis at all, but the oblique adductor hallucis. In answer to this, I can only state that this muscular slip lies in series with the other flexores breves; that, in fact, it is a flexor brevis in every respect except its peculiar nerve supply. It is further to be noted that a large fan-shaped adductor hallucis is present superficial to the short flexors.

But if we extend our inquiries beyond the intrinsic pedal muscles, other deviations may be found from what may be considered the typical nerve supply. In the marsupial *Thylacine* and *Cuscus*, the adductor magnus is entirely supplied by the nerve to the quadratus femoris, and it receives no filaments from the obturator or the great sciatic nerves, as is the case in man.<sup>1</sup> This difficulty might be solved, however, by supposing that the adductor magnus is a compound muscle, consisting of a part belonging to the adductor group, which in these animals has fused with the adductor brevis and a part derived from the flexor group, which in the present instance is independent, and receives its nerve fibres from the same source, but through a different nerve-strand, viz., the nerve to the quadratus.

The adducting group of muscles holds an intermediate position in the thigh, and in man it seems to merge in front with the extensors, and behind with the flexors. The nerve-supply points to this. Thus the pectineus is furnished with twigs from the obturator and anterior crural nerves, whilst the adductor magnus is supplied by the obturator and great sciatic nerves.

<sup>1</sup> Vide Cunningham, "Nerves of the Hind-limb of the Thylacine and Cuscus," *Journal of Anatomy and Physiology*, 1881.

In the same animals the biceps cruris muscle affords another example of a deviation from the usual mode of innervation. This muscle and its accessory parts receive twigs from no fewer than four different sources—viz., (1) the pudic, (2) the nerve to the hamstrings, (3) the external saphenous, and (4) the musculo-cutaneous.<sup>1</sup>

Again, in the ornithorhynchus<sup>2</sup> the outermost belly of the flexor brevis digitorum of the foot is supplied by the external plantar nerve, and in the foxbat the lumbrical muscles of the foot, which are very highly developed, are each furnished with a twig which enters its superficial surface. In the case of the three inner muscles, this twig comes from the internal plantar; but in the case of the outermost lumbrical, the nerve-filament is derived from the external plantar. It is right to mention, however, that the two outer lumbricals in this animal are also supplied by twigs from the external plantar which enter their deep surfaces.

Perhaps the strongest evidence of any against the immutability of nerve-supply is provided by Dr Ruge himself.<sup>3</sup> Thus in the ornithorhynchus he finds the tibialis anticus and the inner portion of the extensor longus hallucis supplied by a branch from the anterior crural, which is prolonged downwards to its destination over the external condyle of the femur. To account for this, he assumes that "the internal part of the extensor longus hallucis and the tibialis anticus are not homologous to the similarly named muscles" in other animals, but "belong rather to the extensor group of the thigh." He believes that the fibres which are supplied by the anterior crural nerve are gradually abolished, and that their place is taken by the external muscles of the leg.

With the first part of this hypothesis I am inclined to agree, viz., that the innervation of these muscles points to their derivation from the extensor muscles of the thigh; but I cannot

<sup>1</sup> Vide Cunningham, "Nerves of the Hind-limb of the Thylacine and Cuscus," *Journal of Anatomy and Physiology*, 1881.

<sup>2</sup> On account of this nerve-supply, Ruge classes this muscular slip with the contrahentis (i.e., adductors). Its connections and position, however, mark it out as belonging without doubt to the flexor brevis digitorum.—Vide Ruge, "On the deep Muscles of the Mammalian Foot," *Morph. Jahrb.*, 1880.

<sup>3</sup> *Loc. cit.* p. 2.

accept the second part of the theory, that the muscles thus derived are replaced by others similarly situated and similarly attached. It is much more reasonable to suppose that the distribution of the peroneal nerve is gradually extended so as to include these muscles; that, in fact, the peroneal nerve invades the territory of the anterior crural in the same manner as we have seen the external plantar nerve encroach upon the internal plantar.

From the facts that I have brought forward, I think that we are entitled to conclude that the doctrine of the invariable relation between nerve-supply and muscle-homology is an erroneous one, and contrary to existing fact. The value of this feature, however, in the determination of the history of a muscle cannot be over-rated. Indeed, it is equalled in importance only by the "insertion." I am tempted to bring forward, from the human body, an illustration to show its importance, and how, if attention had been paid to it, a more consistent nomenclature might have been employed. I refer to the short muscles of the thumb. In our text-books these are described as consisting of an abductor, a flexor brevis (composed of a superficial and a deep head), an opponens, and an adductor. The opponens may be left out of count, seeing that it is a derivative from the outer head of the flexor brevis.<sup>1</sup> These muscles are supplied by the median and the ulnar nerves. The median gives branches to the abductor and the outer head of the flexor brevis. The ulnar supplies the adductor and deep head of the flexor brevis. If we now turn to the foot, we find that both heads of the flexor brevis and the abductor of the great toe are supplied by the internal plantar, which is the median of the pes, whilst the adductor hallucis and transversalis pedis are furnished with twigs from the external plantar or ulnar of the foot. Here, then, is a marked discrepancy, but it is only an apparent one. The truth is, that the adductor pollicis is the serial homologue of the transversus pedis, and the deep head of the flexor brevis pollicis the serial homologue of the adductor hallucis. The abductor and outer head of the flexor brevis pollicis correspond with the abductor and inner head of the flexor brevis hallucis. But where is the true inner head of the flexor brevis pollicis?

<sup>1</sup> *Vide* Ruge on the "Development of the Pedal Muscles," *loc. cit.*

This is a muscular slip, which is almost invariably present; but owing to the great development of the adductors, it has been thrust deeply into the palm. It is the *interosseus primus volaris* of Henle.<sup>1</sup>

Lastly, I consider that it is not at all unlikely—indeed, that it is highly probable—that the source in the brain or spinal cord from which the nerve fibres, destined for the supply of a certain muscle, are derived is invariably the same. Of this, however, we have little proof. It is a matter of certainty, as we have seen, that these fibres may adopt different nerve-strands in order to reach the muscle. Even in the human body great numbers of examples of this may be quoted. Thus the long buccal nerve has been observed by Professor Turner to proceed from the superior maxillary division of the fifth;<sup>2</sup> again, every demonstrator of anatomy has observed the descendens noni taking its origin from the vagus instead of the ninth nerve; and the frequency of the accessory obturator and the accessory phrenic nerves is a fact of common knowledge. The Cetacea afford us a very striking illustration. The absence of functional hindlimbs and the massing of the muscles in the posterior part of the animal into four great columns, which are situated one upon each aspect of the spine, give rise to a corresponding adaptation of the nerves. Thus we find that the lumbo-caudal nerves, after giving off branches to the genitals and the abdominal wall, arrange themselves in four nerve-cords, which extend backwards upon the vertebræ to the tail. Each cord is developed in relation with one of the four fleshy columns.<sup>3</sup> The arrangement is peculiar to the Cetacea, and probably also the Sirenia.

<sup>1</sup> The serial homologies between the short muscles of the thumb and great toe were pointed out more than ten years ago by Bischoff in his elaborate memoir upon the *Hylobates leuciscus*. The author arrived independently at the same results in his researches into the comparative anatomy of the intrinsic muscles of the Mammalian foot.

<sup>2</sup> *Journal of Anat. and Phys.* vol. i., and *Proc. Royal Soc. London*, 1868.

<sup>3</sup> Vide Cunningham, "Spinal Nervous System of Porpoise and Dolphin," *Journal of Anatomy and Physiology*, vol. xi.

## THE ACTION OF DUBOISIA ON THE CIRCULATION.

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IN October 1877, Dr Bancroft of Brisbane gave an account of the mydriatic properties of the Pituri plant before the Queensland Philosophical Society. Mr Tweedy and Professor Ringer subsequently published a joint memoir<sup>1</sup> on the physiological action of this plant, which was soon followed by a contribution by Professor Thos. R. Fraser<sup>2</sup> and by a further paper by Dr Ringer.<sup>3</sup> From these sources all our present knowledge of Duboisia has been obtained.

The Pituri plant, yielding the stimulating narcotic used by the Australian tribes, has been referred to Baron von Müller's *Duboisia Hopwoodii*. From *Duboisia myoporoides* is obtained the alkaloid used by Mr Tweedy and Dr Ringer. The genus *Duboisia* belongs to the family of Salpiglossidæ, a group standing between the Scrophulariaceæ and the Solanaceæ; but in spite of its didynamous stamens it has been relegated to the latter order.

The alkaloid *Duboisia* dilates the pupil, dries the mouth, quickens the pulse, arrests perspiration, produces headache, causes drowsiness, and finally induces tetanus after the lapse of some hours from the time of administration. As might be expected, it acts as an antagonist to muscarin as well as to pilocarpine. It thus appears that *Duboisia* has on the whole a series of actions very similar to those of *Atropia*, and the physiological effects of the alkaloid seem, therefore, to lend a powerful support to the botanical reasons for the classification of the plant along with the Solanaceæ.

<sup>1</sup> *Lancet*, 1878, vol. i. pp. 304-306.

<sup>2</sup> *Proc. Roy. Soc. Edin.* vol. x. pp. 200-202.

<sup>3</sup> *Practitioner*, vol. xxiii. p. 247-249.



In the following investigations the method used by von Bezold and Bloebaum<sup>1</sup> in their observations on Atropia has been taken in some degree as a model. This is of advantage as allowing careful comparison to be made between two drugs which appear to have such close affinities.

All the experiments were conducted with the sulphate of Duboisia, and the influence of the drug on the circulation was in the main estimated by the variations in blood-pressure and pulse-rate as tested by the kymograph and recorded on the revolving cylinder along with time signals from a Ludwig's clock. The experiments were performed while the animals were under the influence of an anæsthetic, and from the observations of the Committee on Anæsthetics of the British Medical Association,<sup>2</sup> the anæsthetic adopted was ether. Contrary to the experience of the committee, however, ether does lower to a certain extent the blood-pressure in rabbits. The fall is not by any means large, it only takes place at the commencement of anæsthesia, and it can be taken account of so easily as to cause no trouble in studying the results of the experiments.

As soon as insensibility was induced a tracheal tube was inserted in order to facilitate the maintenance of anæsthesia by means of etherised air, either by natural or artificial respiration as the case might be. The cannula was then inserted into the left carotid artery, and the drug afterwards injected by the right jugular or left carotid, according as the heart or nervous centres had to be investigated.

Eight experiments were performed on rabbits and several more upon frogs. Each experiment was repeated several times, but only one, a representative one, of each kind is here recorded. The series of experiments falls naturally into the following divisions :—

- I. The influence of Duboisia on the Circulation as a whole.
- II. The influence of Duboisia on the Nervous Mechanism controlling the Heart and Vessels, including—

<sup>1</sup> *Untersuchungen aus dem Physiologischen Laboratorium in Würzburg*, i. Theil, s. 26-58, 1867.

<sup>2</sup> *Brit. Med. Journ.* 1879, vol. i. pp. 12, 108, and 921-923, and 1880, vol. ii. pp. 957-972; also *Journ. of Anat. and Phys.* vol. xiii. pp. 224-231 and 387-396.

1. The intra-cardiac nervous system.
2. The cardio-inhibitory nervous system.
3. The vaso-motor nervous system.

The accelerator nerves have not been taken up. The whole subject is very complicated, and as, according to Schmiedeberg,<sup>1</sup> these nerves are apparently unaffected by drugs which exercise a powerful influence over the other cardiac nerves, it is in the highest degree improbable that Duboisia would have the least effect upon them.

#### I.—EXPERIMENTS WITH DUBOISIA ON THE CIRCULATION AS A WHOLE.

These experiments were conducted without previous interference with the vascular or nervous systems. After placing the kymograph in connection with the left carotid artery, the alkaloid was introduced into the right jugular vein by means of a syringe and cannula.

*Experiment I.*—One milligramme of Duboisia sulphate in the right jugular vein. No previous experiment.

Before injecting, Rate 63 in 15", Pressure 86 mm.					
15"	After	do.	61	do.	92 "
30"	Do.	do.	62	do.	98 "
45"	Do.	do.	63	do.	98 "
1'	0"	Do.	62	do.	94 "
2'	0"	Do.	61	do.	88 "
3'	0"	Do.	63	do.	86 "
4'	0"	Do.	62	do.	94 "
5'	0"	Do.	62	do.	98 "
6'	0"	Do.	62	do.	100 "

At this point the rate and pressure became constant, and remained so until after a little time another experiment caused a change.

<sup>1</sup> *Ludwig's Arbeiten*, v. Jahrgang, s. 41-52.

*Experiment II.*—Two milligrammes of Duboisia sulphate in the right jugular vein. Same rabbit as in previous experiment.

Before injecting, Rate 60 in 15", Pressure 100 mm.

15"	After	do.	56	do.	92	"
30"	Do.	do.	58	do.	98	"
45"	Do.	do.	60	do.	100	"
1'	0"	Do.	60	do.	102	"
2'	0"	Do.	60	do.	106	"
3'	0"	Do.	58	do.	108	"
4'	0"	Do.	60	do.	110	"

From this time no changes in level of the mercury occurred, and only slight differences in rate were observed.

*Experiment III.*—Five milligrammes of Duboisia sulphate in the right jugular vein. No previous experiment.

Before injecting, Rate 62 in 15", Pressure 112 mm.

15"	After	do.	62	do.	109	"
30"	Do.	do.	61	do.	109	"
45"	Do.	do.	61	do.	112	"
1'	0"	Do.	64	do.	114	"
2'	0"	Do.	64	do.	116	"
3'	0"	Do.	65	do.	114	"
4'	0"	Do.	64	do.	116	"
5'	0"	Do.	63	do.	114	"
6'	0"	Do.	62	do.	109	"
6'	45"	Do.	61	do.	99	"
7'	0"	Do.	60	do.	108	"
8'	0"	Do.	60	do.	122	"
9'	0"	Do.	61	do.	122	"
10'	0"	Do.	61	do.	122	"

Between six and seven minutes after the injection of the drug the animal had a severe tetanic spasm, during the occurrence of which several oscillations of the blood-pressure took place. It does not fall within the scope of this paper to take into consideration the action of Duboisia on the nervous system, but it may be remarked in passing, that the drug causes in mammals reflex spasms and convulsions at an early period. It thus approaches the action of Atropia on the spinal nerves, for

the latter alkaloid, as has been shown by Prof. Thos. R. Fraser,<sup>1</sup> induces motor disturbances in mammals before disablement of the peripheral nerves occurs.

*Experiment IV.*—One centigramme of Duboisia sulphate in the right jugular vein. No previous experiment.

Before injecting, Rate 45 in 15", Pressure 110 mm.

15"	After	do.	41	do.	86	"
30"	Do.	do.	42	do.	88	"
45"	Do.	do.	43	do.	90	"
1'	0"	Do.	43	do.	92	"
2'	0"	Do.	46	do.	98	"
2'	45"	Do.	43	do.	100	"
3'	0"	Do.	28	do.	76	"
3'	3"	Do.	—	do.	106	"
3'	15"	Do.	23	do.	60	"
4'	0"	Do.	41	do.	78	"
5'	0"	Do.	41	do.	92	"
6'	0"	Do.	41	do.	104	"
7'	0"	Do.	41	do.	108	"
8'	0"	Do.	42	do.	108	"

In this, as in the preceding experiment, the animal had considerable nervous disturbance. A succession of severe spasms began between two and three minutes after the injection of the drug, and were accompanied by marked variation of rate as well as of pressure.

*Experiment V.*—Two centigrammes of Duboisia sulphate in the right jugular vein. No previous experiment.

Before injecting, Rate 59 in 15", Pressure 100 mm.

15"	After	do.	48	do.	76	"
30"	Do.	do.	50	do.	74	"
45"	Do.	do.	52	do.	76	"
1'	0"	Do.	53	do.	80	"
2'	0"	Do.	56	do.	88	"
3'	0"	Do.	54	do.	96	"
4'	0"	Do.	54	do.	100	"

<sup>1</sup> *Trans. Roy. Soc. Edin.* vol. xxv. part 2, pp. 449-489.

This experiment was at once succeeded, at too short an interval perhaps, by the next.

*Experiment VI*.—Five centigrammes of Duboisia sulphate in the right jugular vein. Same rabbit as in previous experiment.

Before injection, Rate 54 in 15", Pressure 100 mm.

15" After	do.	41	do.	28	"
30" Do.	do.	34	do.	14	"
45" Do.	do.	33	do.	14	"
1' 0" Do.	do.	36	do.	10	"
2' 0" Do.	do.	—	do.	0	"

*Experiment VII*.—One decigramme of Duboisia sulphate in the right jugular vein. No previous experiment.

Before injecting, Rate 60 in 15", Pressure 122 mm.

15" After	do.	41	do.	34	"
30" Do.	do.	18	do.	30	"
45" Do.	do.	—	do.	20	"
1' 0" Do.	do.	—	do.	10	"
1' 15" Do.	do.	—	do.	0	"

Pausing at this point of the inquiry, it will be easy to draw definite conclusions from a retrospect of the work done so far. In the first three experiments the drug caused a considerable rise of the blood-pressure without appreciable effect on the pulse-rate. There was no initial sinking of the mercury in the first experiment, but in the second and third it fell slightly on injection before rising. The last four experiments show that doses of one centigramme and upwards have a tendency to reduce both the pulse-rate and the blood-pressure. After the injection of one or two centigrammes the pressure falls considerably, but afterwards rises to the normal or nearly so. Five centigrammes is a lethal dose, and the pressure rapidly sinks to zero, while the beats of the heart become imperceptible to the stethoscope. A decigramme causes death still more quickly.

The conclusions are obvious. The drug in small doses increases the blood-pressure without materially affecting the constancy of the pulse-rate. In larger doses it reduces both the rate and the pressure, and finally arrests the heart in a state of diastole.

## II. EXPERIMENTS WITH DUBOISIA ON THE NERVOUS MECHANISM CONTROLLING THE HEART AND VESSELS.

### 1. *Experiments upon the Intracardiac Nervous System.*

In the following series of experiments the connections of the heart with the central nervous system were severed in the cervical region before administering the Duboisia. The tracheal tube was placed in connection with the trachea; the cervical spinal cord was then cut between the first and second cervical vertebræ, and artificial respiration commenced. The rabbit was next placed upon the rabbit-frame, and, after section of the pneumogastric and sympathetic nerves, the cannula was inserted as usual into the left carotid artery. The drug was introduced into the right jugular vein. Each injection was only watched for a limited period of time, just enough to permit the alkaloid to pass through the lungs and reach the utmost terminations of the coronary system.

All of these experiments have yet to be repeated on the heart of the frog after the method of Roy.

*Experiment VIII.*—One milligramme of Duboisia sulphate in the right jugular vein. Cord, vagi, and sympathetics cut. No previous experiment.

Before injection, Rate 50 in 15", Pressure 28 mm.

15" After	do.	50	do.	28	„
30" Do.	do.	50	do.	28	„
45" Do.	do.	50	do.	28	„
1' 0" Do.	do.	50	do.	28	„
2' 0" Do.	do.	50	do.	26	„

*Experiment IX.*—Five milligrammes of Duboisia sulphate in the right jugular vein. Cord, vagi, and sympathetics cut. Same rabbit as in previous experiment.

Before injection, Rate 50 in 15", Pressure 28 mm.

15" After	do.	46	do.	32	„
30" Do.	do.	48	do.	28	„
45" Do.	do.	48	do.	28	„
1' 0" Do.	do.	46	do.	28	„
2' 0" Do.	do.	46	do.	28	„

*Experiment X.*—One centigramme of Duboisia sulphate in the right jugular vein. Cord, vagi, and sympathetics cut. Same rabbit as in previous experiment.

Before injection, Rate 46 in 15", Pressure 28 mm.					
15" After	do.	46	do.	25	"
30" Do.	do.	46	do.	25	"
45" Do.	do.	45	do.	26	"
1' 0" Do.	do.	45	do.	27	"
2' 0" Do.	do.	45	do.	28	"

*Experiment XI.*—Five centigrammes of Duboisia sulphate in the right jugular vein. Cord, vagi, and sympathetics cut. Same rabbit as in previous experiment.

Before injection, Rate 45 in 15", Pressure 30 mm.					
15" After	do.	41	do.	24	"
30" Do.	do.	42	do.	26	"
45" Do.	do.	44	do.	28	"
1' 0" Do.	do.	44	do.	26	"
1' 30" Do.	do.	46	do.	26	"

All these experiments were performed on the same animal. We have already seen that Duboisia has little, if any, influence upon the rate of the heart's action when all the nervous connections are intact. It has, as might well be expected, just as little when injected towards the heart with its nervous connections severed. The decided action of the drug on the blood-pressure in the unaltered circulation strongly contrasts with its influence on the arterial pressure after isolation of the heart, when the arterial system also is separated from the control of the general vaso-motor centre. In this series of experiments such doses of the drug as were used appear to be almost destitute of any influence over the blood-pressure. With a dose of one milligramme there is no change in its amount. After the injection of five milligrammes there is a rise of six millimètres, followed by a fall nearly to the initial pressure. One centigramme produces diminution of the pressure and a subsequent rise to the previous level; and finally, after five centigrammes,

there is a fall of six millimetres, succeeded by a rise nearly to the former level.

From these facts we may conclude that Duboisia diminishes the pulse-rate, and in small doses causes a rise, in larger doses a fall, of the arterial pressure. The results of former experiments have shown that the largest doses stop the heart in diastole, and therefore must destroy the functions of the cardiac ganglia and muscle.

## 2. *Experiments upon the Cardio-inhibitory Nervous System.*

The influences of Duboisia on the pneumogastric nerves has been investigated in three ways. In the first place, by injecting the drug towards the brain, by means of the carotid artery, its action on the origin of the nerves, or the inhibitory centres, has been tested. Secondly, the effect of the drug on the cardiac terminations of the vagi has been studied by means of injections towards the heart. Thirdly, the influence of Duboisia on the circulation after section of the pneumogastric nerves has been compared with the results of its administration, without removal of the regulator functions. In analysing the results obtained by all these methods of inquiry, however, it is necessary to bear in mind the well-known fact that the normal restraining influence of the vagi is very slight in the rabbit as compared with many other animals; in fact, it is very often in complete abeyance.

a. The experiments upon the origin of the pneumogastric nerves only differed from the first series described in this, that the cannula to be afterwards connected with the syringe was inserted into the peripheral part of the left carotid artery instead of the central part of the right jugular vein. It is only necessary to detail one experiment.

*Experiment XII.*—Five milligrammes of Duboisia sulphate in the left carotid artery. No previous experiment.

Before injection, Rate 63 in 15", Pressure 120 mm.			
3" After	do.	65	do. 122 "
6" Do.	do.	50	do. 118 "



9"	After injection, Rate 45 in 15", Pressure 112 mm.				
12"	Do.	do.	60	do.	110 "
15"	Do.	do.	65	do.	112 "
18"	Do.	do.	65	do.	114 "
21"	Do.	do.	65	do.	116 "
24"	Do.	do.	65	do.	118 "
36"	Do.	do.	65	do.	120 "
48"	Do.	do.	65	do.	120 "
1' 0"	Do.	do.	65	do.	120 "

In this experiment there is quite a remarkable diminution in the rate of the heart's action immediately succeeding the introduction of the drug. It is attended by a fall of arterial pressure more gradual in character. The former attains its minimum rate about nine seconds after injection; the latter falls to its lowest level about twelve seconds after injection. The lowering is only transient, being speedily succeeded by a very rapid return to the normal rate of pulse, and by a much more gradual rise to the normal pressure. The explanation of these facts appears to be simple. The primary effect following injection at once is a stimulation of the regulator centres or the origin of the vagus nerves. This shows itself by increased inhibition, in consequence of which the slowing as well as the fall is brought about. But at that period of time when the drug has just reached the heart and completed the pulmonary circuit, the inhibitory influence is succeeded by something causing a return to former conditions. It has been shown by von Bezold and Bloebaum that atropia paralyses the cardiac terminations of the vagi; and the simplest explanation of this abrupt cessation of inhibition is that Duboisia, in so many other respects closely resembling Atropia, has the same action, and suspends the influence of the cardio-inhibitory apparatus by paralysis of the cardiac ends of the vagi.

b. The experiments about to be described form a very definite and exact source of information. After applying the kymograph the vagi were cut in the neck. The peripheral portion of the right vagus was then laid upon ordinary metallic electrodes connected with the secondary coil of a Du Bois-Reymond's induction apparatus, driven by a medium-sized bichromate element. An electric stimulus was given in this way to the

cardiac end of the divided nerve, which served to show the extent of inhibition which was caused. A dose of Duboisia was then injected into the right jugular vein, and the electric stimulus again given, for comparison of its effects with the result following stimulation before the administration of the alkaloid. The details of the experiments are as follows:—

*Experiment XIII.*—One milligramme of Duboisia sulphate in the right jugular vein. Stimulation of the right vagus. No previous experiment.

Before section of vagus, Rate 64 in 15", Pressure 106 mm.					
15" After	do.	do.	64	do.	86 "
30" Do.	do.	do.	65	do.	97 "
45" Do.	do.	do.	66	do.	102 "
1' 0" Do.	do.	do.	67	do.	95 "
1' 15" Do.	do.	do.	67	do.	90 "
1' 30" Do.	do.	do.	65	do.	86 "
1' 34" Do.	do.	do.	Nerve stimulated.		
1' 35" Do.	do.	do.	Rate 20 in 15", Pressure 50 "		
1' 40" Do.	do.	do.	Current stopped.		
1' 45" Do.	do.	do.	Rate 65 in 15", Pressure 76 "		
2' 0" Do.	do.	do.	65	do.	90 "
2' 15" Do.	do.	do.	67	do.	96 "
2' 15" Do.	do.	do.	Drug injected.		
2' 30" Do.	do.	do.	Rate 64 in 15", Pressure 96 "		
2' 45" Do.	do.	do.	69	do.	100 "
2' 45" Do.	do.	do.	Nerve again stimulated.		
3' 0" Do.	do.	do.	Rate 67 in 15", Pressure 102 "		

The nerve was stimulated after section with the secondary coil separated from the primary by a distance of eighty millimetres. After injecting the Duboisia, the same strength of current was used; but when no response followed, the secondary was approximated to the primary coil, in order to derive the strongest possible stimulus. It was of absolute inutility, as the tracing showed. It appeared to be of interest to discover whether a still smaller quantity of the drug than that used above might have sufficient power to paralyse the cardiac terminations of the vagi, and the next experiment was performed to determine this point.

*Experiment XIV.*—One-tenth of a milligramme of Duboisia sulphate in the right jugular vein. Stimulation of the right vagus. No previous experiment.

Before section, Rate 80 in 15", Pressure 135 mm.					
15" After	do.	80	do.	130	"
30" Do.	do.	80	do.	128	"
45" Do.	do.	80	do.	127	"
1' 0" Do.	do.	80	do.	127	"
1' 11" Do.	do.	Nerve stimulated.			
1' 15" Do.	do.	Rate 33 in 15", Pressure 75			
1' 16" Do.	do.	Current stopped.			
1' 30" Do.	do.	Rate 78 in 15", Pressure 124			
1' 45" Do.	do.	78	do.	126	"
1' 45" Do.	do.	Drug injected.			
2' 0" Do.	do.	Rate 77 in 15", Pressure 132			
2' 15" Do.	do.	78	do.	135	"
2' 15" Do.	do.	Nerve again stimulated.			
2' 30" Do.	do.	Rate 78 in 15", Pressure 135			

The same result therefore followed the administration of 0·0001 gramme of Duboisia sulphate as that which has been shown above to result from 0·001 gramme.

Duboisia thus possesses a specific action on the peripheral inhibitory apparatus, which is brought into a state of paralysis probably more rapidly by this drug than by any other. Only thirty seconds were allowed to elapse between the injection of the Duboisia and the stimulation of the nerve, yet this was quite sufficient for even so minute a dose as the tenth of a milligramme.

c. From a knowledge of the facts just detailed, it would naturally be expected that after section of the vagi Duboisia would produce less change in the working of the circulation than when administered without previous elimination of the inhibitory influence. It is so sometimes,—not always; for the caution previously given must be borne steadily in view that the normal inhibitory influence in the rabbit is at all times slight, and often absent. Even in the few experiments detailed in this paper marked contrasts are presented as the results of division of the vagi; in some cases the rate and pressure

respectively rise and fall ; others show the converse, *i.e.*, the rate diminishes while the pressure is augmented ; and yet again in others both rate and pressure appear but little affected. In an animal whose regulator mechanism is dormant the introduction of a substance which paralyses the cardiac terminations of the pneumogastric will have the same result whether the paths of inhibitory mandates be intact or severed. If, on the contrary, the circulation is more or less under the control of the inhibitory nerves there will be a difference according as the nerves are or are not divided. For these reasons little stress can be laid upon any facts drawn from administration after section of the nerves.

As a matter of experience, however, the changes of pressure following injection of the drug after division of the nerves have never been so great as those caused by the alkaloid without such section. This, of course, only tells us that the greatest results follow the administration of Duboisia when, in addition to its action on other factors shortly to be discussed, the drug paralyses an existing degree of cardiac inhibition.

### 3. *Experiments upon the Vaso-motor Nervous System.*

*a.* The method adopted to investigate the action of Duboisia on the vaso-motor centres was, in the first place, to divide the pneumogastric nerves in the neck, so that there could be no inhibitory disturbances, and afterwards to inject the drug into the peripheral part of the carotid artery.

*Experiment XV.*—Five milligrammes of Duboisia sulphate in the left carotid artery. Previous division of the vagi. No previous experiment.

Before injection, Rate 46 in 15", Pressure 54 mm.					
3" After	do.	—	do.	70	„
6" Do.	do.	—	do.	74	„
15" Do.	do.	50	do.	68	„
30" Do.	do.	50	do.	78	„
45" Do.	do.	50	do.	80	„
1' 0" Do.	do.	50	do.	82	„
1' 30" Do.	do.	50	do.	84	„

An instantaneous and considerable rise of pressure, accompanied by slight quickening of the rate of pulsation, occurred on injection, and before the drug could reach the heart. The increase of pressure was large, the column of mercury ninety seconds after administration being rather more than one and a half times as large as previous to injection.

*Experiment XVI.*—One centigramme of Duboisia sulphate in the left carotid artery. Previous division of the vagi. Same rabbit as in last experiment.

Before injection, Rate 50 in 15", Pressure 84 mm.				
3" After	do.	—	do.	94 "
6" Do.	do.	—	do.	104 "
15" Do.	do.	48	do.	76 "
30" Do.	do.	48	do.	86 "
45" Do.	do.	48	do.	86 "
1' 0" Do.	do.	48	do.	84 "
1' 30" Do.	do.	50	do.	78 "

During the first six seconds the rise of pressure was exactly equal to that of the same period in the previous experiment; but this was immediately succeeded by a fall. The pulse-rate showed a slight diminution.

*Experiment XVII.*—Five centigrammes of Duboisia sulphate in the left carotid artery. Previous division of the vagi. Same rabbit as in the last experiment.

Before injection, Rate 50 in 15", Pressure 80 mm.				
3" After	do.	—	do.	86 "
6" Do.	do.	—	do.	86 "
15" Do.	do.	49	do.	76 "
30" Do.	do.	49	do.	72 "
45" Do.	do.	51	do.	72 "
1' 0" Do.	do.	52	do.	66 "
1' 30" Do.	do.	52	do.	62 "

This experiment showed a still greater downward tendency, the initial rise of pressure even being small.

From these experiments there can be no doubt that Duboisia has a stimulating effect on the vaso-motor centres, by means of

which the blood-pressure is increased. This is the primary effect of the few seconds immediately following injection of all doses; but the final result depends upon the amount of the drug administered. When the quantity is small there is a permanent rise of pressure; when, on the contrary, it is large, a decidedly lowering effect is produced. The quantity required to show this downward tendency is just about the same as that shown to cause a fall in the first series of experiments, *i.e.*, between five milligrammes and one centigramme.

In order to study the manner in which the rise of pressure occurs, the web of the frog's hind foot has been repeatedly examined with the greatest care. In doing this, however, the investigation was approached with a decided bias. After knowing from the experiments just described that the pressure undergoes augmentation on the injection of Duboisia towards the vaso-motor centres, it was only natural to expect a diminution in the size of the arterioles. It is well to confess this, that it may be distinctly understood how difficult it has been to be absolutely certain beyond all question that it was not merely a reflection of what was expected which was seen.

After the injection of any quantity of Duboisia not exceeding five milligrammes into the cellular tissue of the frog, there is a contraction of the arterioles of the web; with doses above a centigramme there is, on the contrary, a diminution of their calibre.

No absolute weight can be allowed to these observations. In such investigations, where the changes in the size of the vessels are so small, there is always too much room for the play of the imagination. The testimony of the experiments lately described, however, is amply sufficient to prove that Duboisia has a clear and definite action on the vaso-motor centres.

Before leaving this part of the subject it should be stated that the rise following the injection of Duboisia towards the brain cannot be attributed to psychical disturbance. In this series of experiments, as in all the others, the animals were in a uniform condition of anæsthesia, and gave no indication of nervous irritation.

b. The rabbit's ear affords an admirable subject, by means of which the influence of Duboisia on the sympathetic nervous

system can be studied; for comparison of the effects of stimulation of the divided nerve in the neck before and after administration of the drug shows whether any alteration is caused by its use.

*Experiment XVIII.*—Five milligrammes of Duboisia sulphate in the right jugular vein. Stimulation of the sympathetic in the neck. No previous experiment.

The right sympathetic nerve of a rabbit was divided in the neck, the other being left intact for the purpose of comparison. The right ear at once became markedly hyperæmic, and formed a decided contrast to the left. The upper end of the divided nerve was then stimulated by means of the induced current. As in previous experiments, the primary current was from a medium-sized bichromate cell, and the secondary was separated from the primary coil by a distance of thirty millimetres. The right ear paled at once, and on the cessation of the current a vivid flush again overspread the ear. The drug was then injected, and five minutes afterwards the nerve was again stimulated. Pallor of the ear resulted just as before, demonstrating beyond question that Duboisia has no tendency in small doses to destroy the functions of the sympathetic nerves. Another rabbit was used to study the effects of larger doses.

*Experiment XIX.*—Two centigrammes of Duboisia sulphate in the right jugular vein. Stimulation of the sympathetic in the neck. No previous experiment.

The various steps in this experiment differed in no respect from those just described, and the results were precisely the same. It is quite unnecessary, therefore, to detail them.

These experiments conclusively prove that Duboisia has no paralysing action on the sympathetic nerve in any part of its course; but they leave it open to question whether the drug may not have a tendency to heighten the excitability of the nerve, and augment its action upon the smaller branches of the arterial system.

Such are the methods of observation pursued, and the results obtained, in this investigation into the action of the alkaloid of

the Pituri plant. The inquiry is not yet by any means complete; but, as an abstract of the work was sent up in April of this year to the International Medical Congress, and as the paper was read there in August, the results obtained so far must now be put forward with the proviso that certain parts—as, for instance, the observations on the cardiac ganglia—need amplification and revision.

Finally, the author would record his great obligations to Professors Thos. R. Fraser and Rutherford and to Drs Birch, Cunningham, Hay, and Haycraft, for constant and unwearied help of every kind.

#### CONCLUSIONS.

1. Duboisia in quantities not exceeding 0.005 gramme raises the arterial blood-pressure without materially affecting the pulse-rate.

2. In quantities not exceeding 0.05 gramme it diminishes the blood-pressure and lessens the pulse-rate.

3. In quantities of 0.05 and upwards it causes death, with the heart in a state of diastole.

4. Upon the heart itself Duboisia has but little action, except in very large doses, *i.e.*, doses of more than 0.05, and then it causes arrest of the heart in diastole.

5. Duboisia stimulates the central inhibitory mechanism.

6. The alkaloid paralyses the peripheral inhibitory apparatus.

7. Duboisia stimulates the central vaso-motor apparatus, and causes contraction of the arterioles in small doses; in large doses it lowers the activity of the central vaso-motor mechanism and dilates the arterioles.

8. Duboisia has no influence over the sympathetic nerve.



ON THE CEREBRAL SINUSES AND THEIR VARIATIONS. BY J. F. KNOTT, F.R.C.S.I.

THE peculiarities in the cerebral circulation are so numerous and striking as to be obvious even to the most superficial observer. The most prominent were known to the older anatomists, but it is not a little remarkable how imperfectly they must have been examined, as some of the mistakes which are gravely put forward in their descriptions are absolutely ludicrous when read by the light of our present knowledge.

The veins of the brain being of large size and superficially situated at once meet the eye of the anatomist, and require comparatively little trouble for an investigation of their course. The most obvious peculiarities presented by these vessels when contrasted with the various canals of the other parts of the body are: the looseness of their connections with the surrounding tissues; their taking a course completely separate from that of the arteries; and their terminations in the peculiar dura-matral canals, into which they pass (at least in the case of the large proportion which open into the superior longitudinal sinus) in a direction opposed to that of the current within the receiving vessel. The absence of valves is a characteristic which they possess in common with most of the veins of the other cavities of the body.

The sinuses or dura-matral canals which receive the cerebro-meningeal veins were described with considerable accuracy by Galen, and it is curious to find that Vesalius, in times comparatively modern, and with better opportunities for examination of the human body, should have retrograded so much as to contradict Galen's account by asserting that the sinuses received arteries as well as veins, and that a pulsation was transmitted from the former. The mistake of Vesalius was refuted by Fallopius, but again revived and adopted by Vieussens, Wepfer, and other anatomists of this period, who were imposed upon by the facility with which injected fluid passed from the arteries into the sinuses. The minute and careful dissections of Albinus with the microscopic observations of Lieuwenhoek, finally settled the question, and established the origin of the veins from

the capillaries at the termination of the arteries, in a way similar to the arterio-venous connection in other parts of the body.

These venous channels are peculiar in their outline, the deficiency of muscular and scantiness of elastic fibres in their walls, the absence of valves, the presence of tendinous strings, and, in some situations, of Pacchionian excrescences in their interior, and the contrariety of direction of a large proportion of their chief tributaries.

These blood-channels lie in the thickness of the dura mater, and are lined by a layer of endothelial cells prolonged from the internal membrane of the arteries and capillaries. Outside this is a thin layer of elastic fibres longitudinally arranged, while the remaining thickness of the coats is formed by the dura mater itself. In case of the greater number one part of the circumference of the vessel corresponds to the bone which is grooved for its accommodation. The layer of dura-matral fibres which lies next the lumen of the canal is in most cases longitudinally arranged, while a circular layer surrounds these, or, in many instances, a series of layers of circular and longitudinal bundles, which are exceptionally poor in yellow fibres. The individual and local varieties observable in the structure of the walls of the sinuses are very numerous, so that the description just given can only be regarded as that of the prevailing arrangement. A marked internal peculiarity is found in the existence of the so-called *Chordæ Willisii* which cross the lumen of some of these canals—the superior longitudinal, the cavernous, and the lateral. In the first-mentioned canal these form pretty strong tendinous cords; in the other sinuses they present a softer consistence and reddish colour. From the walls and Willisian cords of the cavernous sinuses little club-shaped villous processes project into the interior of its cavity (Hyrtl). There are some of about 2 millimetres in length, and a much greater number of about half.

The greater proportion of the veins from the convexity of the cerebral hemispheres enter the superior longitudinal sinus, and present the remarkable peculiarity that the direction of the majority is the reverse of that of the current of blood within the sinus itself. These veins, of which there are about twelve to fifteen on each side, pass obliquely backwards, and, as Lower long ago

observed, in most instances (although there are always a few exceptions), they pass obliquely between the walls of the sinus before opening into it like the termination of the gall-duct in the intestine or the ureters in the bladder. Ridley asserted that one-half of the tributaries of the sinus longitudinalis superior opened backwards.

Small masses of areolar tissue connect the pia mater to the dura mater for a little distance around the termination of each of these veins, and this connection arrested the attention of many of the older anatomists. Ridley calls them "*carneous adnescences*." Vieq d'Azyr confounds them with the glandulæ Pacchioni. Sir Charles Bell thus describes the intermembranous connection: "it is not a simple adhesion of the pia mater and dura mater; but a white spongy substance seems to connect and strengthen them, and, when torn asunder, it leaves a soft fatty kind of roughness upon the pia mater." Other small elevations or thickenings appear here and there along the falx cerebri, at or near the wall of the superior longitudinal sinus, but they are not constant or regular. Both these and the other spongy masses above mentioned have been erroneously included under the name of glandulæ Pacchioni. The structures which Pacchioni specially described, and which engaged him in a series of violent disputes with Fautonus, are situated *within* the cavity of the superior longitudinal sinus, and connected with the openings of the veins. He says of them "*ovorum instar bombycinorum apparent*," which describes their peculiarity of shape; he also notices their pale fleshy colour, which he attributed to an investment of muscular fibres. "*In longitudinale sinu, immediate sub membranosis expansionibus, in areolis chordarum Willisianarum, quin et supra easdem chordas, consitæ sunt innumeræ glandulæ conglobatæ, propria, et tenuissima, membrana, veluti in sacculo conclusæ; quæ racematine ut plurimum cœunt; raro sparsine disponuntur: hæ glandulæ utrinque ad latera falcis messoriæ, ab ejusdem apice ad hasis usque posticam partem miro prope modum artificio procedentes, dorso læertorum accumbunt, et partim ab horum fibris, partim ab iis quæ a chordis emergunt, firmantur, atque invicem alligantur, ita at non nisi lacerat acu disjungi possint.*" Pacchioni believed that the ducts of these glandular bodies

passed to the pia mater, and conveyed a secretion which lubricated the surface of the brain. He also suggested that they were pressed, and their secretion promoted, by the motion of the chordæ Willisianæ and the action of the dura mater. Fautonus, who opposed Pacchioni's views so strongly, was of opinion that they gave out a fluid into the sinus which diluted the venous blood.

Sir Charles Bell suggested "that they had a valvular action on the mouths of the veins; they project from the mouths of the veins in the sinus, and the blood passing from the veins must filter through them, and be checked in its retrograde course." It is unnecessary to add that these views can now be of interest only to those who are curious to be skilled in the legendary lore of anatomical literature.

Turning to the consideration of the anatomical arrangement of the sinuses themselves, which forms the more immediate subject of this communication, we shall take as the starting-point the *confluens sinuum*, or so-called **torcular Herophili**, to which the great bulk of the intracranial blood is conveyed before it takes its definite downward course to the foramina of exit at the base of the skull. This cavity forms the point of union of the lateral, straight, occipital, and superior longitudinal sinuses, and is placed at the crucial spine of the occipital bone, in the majority of cases inclining a little more to the right side. Of those which I specially examined, twenty-seven specimens of forty-four were more to the right side, eight were mesial in position, nine had a slight inclination to the left. It forms an irregular cavity, the walls of which are mutually connected by strong Willisian chords. The ancient anatomists, ignorant of the true direction of the circulating blood, believed that the vital fluid, previous to passing into the brain, here underwent some mysterious process of elaboration, by which it was specially prepared for the nutrition of this important organ. "Cœuntes autem in vertice capitis, quæ sanguinem deducunt meningis duplicaturæ, in locum quendam vacuum quasi cisternam (quem sane ob id ipsum Hierophilus *torcular* solet nominare), inde velut ab arce quadam omnibus subjectis partibus vivos mittunt; quorum numerum nemo facile dixerit, quod partium nutriendarum numerus sit infinitur (*Galen, cap. vi. De torcular*).

The right-sided inclination of the torcular Herophili, as well

as the usually larger size of the right lateral sinus, were attributed by Lower, in comparatively modern times, to the practice generally recognised among nurses of usually laying children on the right side.

Other names which the imaginative nomenclature of the old anatomists has furnished to this venous cavity are *lacuna*, *platea*, *laguncula*, *pelvis*, *polmentum*, *tertia vena*, &c.

In the following notes the normal arrangement is compared with the variations observed in forty-four cases, in which the sinuses were carefully dissected:—

**Sinus lateralis;** *s. transversus* (Henle); *s. sigmoides* (M. J. Weber); *s. tentorii posterior*.—This vessel receives as its tributaries the chief of the other sinuses, and is the great feeder of the internal jugular vein. It commences at the internal protuberance of the os occipitis, and, taking at first a horizontal course, passes outwards along the transverse part of the linea cruciata on the corresponding side. On leaving the occipital bone, it passes on to the posterior inferior angle of the parietal, from which it passes over the inner surface of the mastoid portion of the temporal bone, to groove the occipital a second time on the upper surface of its jugular process. Bending into the jugular foramen, it there joins the inferior petrosal and marginal sinuses to form the internal jugular vein. The transverse portion of the sinus presents the outline of a triangular prism, as it lies between the layers of the tentorium cerebelli. In the remaining part of its course it lies beneath the dura mater, and a transverse section is of semicircular form. The diameter of the vessel is considerable, averaging 8–10 mm. (W. Krause), but there is often a great inequality between those of opposite sides. The right is very generally the larger, but I have myself met with two instances of its almost complete absence, only a small venous canal of  $1\frac{1}{2}$  mm. diameter following its course as far as the mastoid foramen, through which it disappeared. In the majority of cases the right is also longer than the left, and in four cases of the forty-four in which these sinuses were carefully examined, the superior longitudinal turned directly into the right lateral sinus, which appeared to be a direct continuation of the other. In these cases the left was only about one-third the size of the right lateral sinus.

*Varieties.*—The cases of diminutive size of the right lateral sinus have been already mentioned. Lieutaud has recorded a case of complete absence of the left lateral sinus, as far as the usual point of entrance of the superior petrosal sinus. The continuation of the latter with some tributary veins formed a diminutive representative of the lower part (*Essais Anat.* p. 332.) Both lateral sinuses may be small, and the greater part of the blood be transmitted along the occipital and marginal sinuses to the foramen jugulare to join the internal jugular vein. Henle alludes to this abnormality; there occurred one good example among the cases which I examined. A horizontal septum sometimes crosses the cavity of the sinus through a portion of its length, or even along the whole. In the latter case the channel is divided into two. Of this I have met two specimens, both occurring on the right side. This peculiarity was also observed by Hallett, who reported two examples (*Med. Times*, 1848).

*Additional Branches.*—*Emissarium occipitale.*—This venous canal passes through the occipital protuberances, communicating on the outside with the occipital veins, and on the inside with the *torcular Herophili*. A small vein could be distinctly traced through the bone in several cases (six in forty-four). In most of the others, there was found in this situation a small vein piercing either table of the occipital bone, and anastomosing with the diploic veins between, in this way representing the more distinct communication already mentioned.

An accessory sinus has been described by Kelch, which passes from the sphenoidal fissure backwards, crossing the upper border of the petrous portion of the temporal bone beneath the tentorium cerebelli, and beneath the superior petrosal sinus, with which it anastomoses in this situation. It then passes back on the under surface of the tentorium to join the lateral sinus, in which it ends (Kelch, *Beiträge zur Pathol. Anat.* 80). In front it communicates with some of the veins of the orbit. I have been able to trace a small vein along this course in three instances.

*Vena aberrans.*—Under this name Verga describes a channel of communication between the sinus cavernosus (or vena ophthalmica) in front, and the sinus lateralis sinister behind. I once found a vein occupying the corresponding position on the right

side, but have not met with the vessel which Verga describes on the left. (Verga, *Annal. Univ. di Med.* 1856).

*Sinus ophthalmopetrosus* (Hyrstl).—Under this name Hyrstl describes a small occasional sinus which passes backwards from the sphenoidal fissure over the inner surface of great wing of sphenoid and anterior surface of petrous portion of temporal bone, to terminate in the lateral sinus. It sometimes leaves the skull through the foramen ovale, and in other cases it usually communicates with the middle meningeal veins. In four cases of the forty-four in which the sinuses were specially examined for variations, I was able to trace this vessel.

*Sinus squamoso-petrosus* (C. Krause), s. *petroso-squamosus* (Luschka).—This small vessel, when present, occupies the angle between the petrous and squamous portions of the temporal bone, and opens into the lateral sinus after crossing the posterior extremity of superior border of petrous bone, or passing through a canal in the latter. In front it pierces the squamous portion of the temporal bone, a little above the root of the zygomatic process, or perhaps the latter process itself, to communicate with the deep temporal veins.

In some cases it may be found to communicate with a small accessory sinus which passes through a canal in the bone, leading from the lower part of the sulcus transversus ossis occipitis to the mastoid foramen (Otto, *Seltene Beobachtungen*, 1824, ii. 70). This canal corresponds to the *canalis temporalis*, whose existence is normal in some mammals (Otto, *Nov. Act. Acad. Cæs. Leop. Carol.* 1826, xiii. 23). This canal and contained vein I have found well developed in two instances only. As was first pointed out by Rathke, the study of the development of this bone throws additional light on the homology of the *canalis temporalis*, as a "*foramen jugulare spurium*," is found to be a normal opening in the corresponding part of the bone, during a great part of the embryonic period of the life of the human being.

This vessel (*sinus squamoso-petrosus*) is described by Sir Charles Bell under the name of the anterior petrous. He seems to have been familiar with its existence, and to look upon its occurrence as frequent; and it is strange that, like many other important venous canals, its existence should be ignored by the authors of so many of our anatomical hand-books. In the cases

which I selected for examination, seven of the forty-four bodies presented such a vessel on both sides, and nineteen others on one side only. Of the unilateral specimens eleven occurred on the left side, and eight on the right.

**Sinus longitudinalis superior**; (*s. sagittalis superior*; *s. falciformis superior*; *s. triangularis*).—This reaches from foramen cæcum in front to internal occipital protuberance behind; or, as oftener happens, it turns, immediately before its termination, a little to one side to join one of the lateral sinuses, usually that of the right side. Through the foramen cæcum it communicates in early life with the veins of the nasal fossæ, but this anastomosis is usually cut off in the adult. In its course it occupies the median plane of the skull, passing backwards between the layers of the dura mater, which splits to enclose it, along the crista frontalis, sulcus frontalis, sutura sagittalis, and upper branch of the linea cruciata, on inner surface of occipital bone. Of thirty-eight calvaria on which I carefully examined the position of the groove which marked the course of this canal, I found it to occupy the median line as accurately as possible in nine specimens. In seven other cases it deviated only at the posterior extremity, terminating in the right lateral sinus in four, and in that of the left in three instances. In fourteen specimens the sulcus was more on the right side along the whole length of the sagittal suture, but in three of them it bent to the opposite side on the occipital bone, at about an inch and a half above the protuberance, to terminate in the groove for the lateral sinus. In two the termination was medial; in the others it was on the same side as the original deviation. In the remaining eight the sagittal groove was better marked on the left parietal bone, and in six it terminated in the corresponding lateral groove of occipital. In one case it gained the median line at the protuberance, and in one it crossed to the opposite side. The lumen of this sinus presents a distinctly triangular outline with a slightly concave base which is placed above, and its diameter gradually increases as it passes backwards, from 1.5 mm. at the level of the apex of the crista galli to 11 mm. at its termination. The diameter of the sinus is, however, subject to very great variations, the inconstancy of size being, indeed, the most striking feature in its abnormal anatomy. In some cases a



second small sinus may be found between the layers of the falx immediately below this normal superior longitudinal, or, in such cases it would be more accurate to describe the sinus as divided into two by a horizontal septum. The upper is thus quadrilateral in section; the lower is triangular with the apex downwards. The canals so arranged have been described by Malacarne under the name of *seni subalterni*. I have seen an instance of this division; it reached along the posterior half of the sinus only.

*Varieties.*—A case of complete absence of the superior longitudinal sinus is mentioned by Portal (*Cours d'Anat. Med.* iv. 29). This must, of course, be a very rare anomaly. I have not met with an instance, but I have certainly seen three specimens (out of forty-four in which it was carefully examined), in which the size was so small that it could hardly be looked upon as a representative of the normal sinus. The usual tributaries in these cases turned down between the layers of the falx cerebri, and joined the inferior longitudinal sinus, while the normal tributaries of the superior petrosal sinuses were larger than usual. In two the straight sinus received the termination of the superior longitudinal, which did not groove the occipital bone for the last inch of its course, but turned a little forwards as well as downwards between the layers of the falx.

The superior longitudinal sinus was found to bifurcate in two of the forty-four; in each case a little behind the coronal suture. The two branches re-united after a course of an inch and a half, in this manner enclosing a narrow ellipse. A similar case was reported by Vicq d'Azyr (Tab. xxxii.) Malacarne has recorded a case in which the superior longitudinal sinus bifurcated on reaching the apex of the occipital bone, and either branch followed the corresponding limb of the lambdoid suture to the junction with the temporal bone, when it joined the lateral sinus. The lateral sinus was then small up to that point. I have not met with an example of this strange anomaly.

*Sinus longitudinalis inferior* (*s. sagittalis inferior* vel *minor*; *s. falciformis inferior*; *vena longitudinalis inferior*). This canal (which hardly deserves the name of sinus as it more nearly resembles a vein in form) commences at a variable distance behind the anterior extremity of the falx cerebri, and passes backwards between the layers of this process of dura mater till

it reaches the anterior margin of the tentorium cerebelli where it joins the *sinus rectus*.

*Varieties*.—I have hardly found any notable abnormality in this sinus in any of the cases examined, the variations being confined to change of size and irregularity of position of origin. In one case, however, its termination was very peculiar. Instead of passing into the *sinus rectus*, it turned upwards between the layers of the falx to terminate in the superior longitudinal sinus, an inch and a quarter above the level of the internal occipital protuberance.

*Sinus rectus*; *s. quartus* (Galen); *s. perpendicularis* (Haller); *internal sinus* (Sir C. Bell); *s. obliquus*; *s. tentorii medius* (M. J. Weber).—This vessel, which is formed by the confluence of the inferior longitudinal sinus with the vena magna Galeni, passes backwards and downwards to join the *torcular Herophili*, or (what oftener happens) it may turn a little to one side to pass into one of the lateral sinuses. "It opens, for the most part, by an oval mouth, formed by strong pillars of fibres, into the left lateral sinus, rather than directly in the middle of the communication of the three great sinuses." Such is the description of its termination given by Sir Charles Bell, and it is entirely in accordance with my experience. In twenty-six cases out of forty-four it opened into the left lateral sinus, the terminal orifice answering exactly to the description above quoted. In six cases the deviation was to the right, while it ended medially in twelve instances. A vertical horizontal section at any part of its length presents a triangular outline with the base below, and apex above. The base is formed by the tentorium itself, while the sides are formed by the splitting of the layers of the falx cerebri. The diameter of this triangular canal is about 3-4 mm. for the greater part of its length, but is found to vary a good deal.

*Varieties*.—In one case I found this sinus completely absent. The vein of Galen and the inferior longitudinal sinus met at the anterior edge of the tentorium as in the normal arrangement, but the fusion was only for a length of about three quarters of an inch. From this point three veins passed backwards, one between the layers of the falx cerebri inclined upwards to join the superior longitudinal sinus an inch above the torcular, while the other two lay between the layers of the tentorium on the left side of the

attachment of the falx. One opened into the left lateral sinus half an inch beyond the margin of the falx; the termination of the outer one was an inch farther from the middle line.

*Sinus sphenoparietalis* (Breschet); *sinus alæ parvæ*; *superior sphenoidal sinus* (Sir C. Bell).—This vessel arises from one of the meningeal veins at the outer extremity of the lesser wing of the sphenoid bone, and passes inwards under cover of the latter to terminate in the cavernous sinus. Its position is marked by a slight groove in the bone. It is of considerable dimensions, the diameter near its inner end being about 3 mm. I have observed great variability in the size of this venous canal, but in no case did I meet with an instance of its complete absence, a small vein at least was to be found in those cases where the normal sinus was not fairly represented. Accordingly I am surprised that a description is so often omitted from our textbooks.

*Sinus cavernosus*; *receptaculum*; *s. caroticus* (Rektorzik); *confluens sinuum anterior*; *s. sphenoparietale* (Cruveilhier).—This is a space of extremely irregular outline, inclosed between the layers of dura mater on the lateral aspect of the body of the sphenoid bone, immediately above the root of the great wing. As already mentioned, it is crossed by fibrous trabeculæ from some of which villous process hang into the current of venous blood. In the layer of dura mater which forms its outer wall, several important nerves are included, while within the cavity, and in contact with the inner wall, is the carotid artery, which is separated from the venous canal only by the lining of the latter membrane.

Ridley describes in these words: "Another I discovered having the veins injected with wax, running round the *pituitary gland* on its upper side, forwardly within the duplicature of the dura mater, backwardly between the dura mater and pia mater, then somewhat loosely stretched over the subjacent gland itself, and laterally in a sort of canal made up of the dura mater above; and the carotid artery on each outside of the gland, which, by being fastened to the dura mater above, and below at the basis of the skull, leaves only a little interstice betwixt itself and the gland." Brunnerus describes this sinus.

*Varieties*.—An example of complete absence of this sinus is

mentioned by Santorini (*Obs. Anat.* 1714, cap. iii. § 25). This I have not observed, but in some of those which I examined it was so small that it could hardly be looked on as a true representative of the normal cavernous sinus. This occurred in five cases, three of which were on the right side and two on the left.

An additional tributary, in the form of an emissary vein passing through the *canalis rotundus* of sphenoid with the superior maxillary nerve, has been described by Nuhn. This I have seen twice; in both cases on the right side. Another inconstant vein which lies in the dura mater on the inner surface of the great wing of the sphenoid bone, has been described by Sir C. Bell and some other anatomists as the *inferior sphenoidal sinus*. It was present in twenty-three of the forty-four subjects, fourteen on the right, nine on left side.

**Sinus intercavernosus.**—The receptacula of opposite sides are connected by means of one or more transverse vessels which cross the pituitary fossa. There are frequently two, one in front of the hypophysis, and the other behind. The anterior is the larger, the posterior is more often completely absent. Its absence was noted in twenty-six of my forty-four cases. In two of the others the posterior branch was the larger of the two, and in another case it was the only one present. In fifteen instances only did the above described arrangement of two transverse branches exist. When present they form with the cavernous sinus on each side the so-called

**Sinus circularis** (Ridley); *s. ellipticus*; *s. coronarius*; *clyneid sinus* (Sir C. Bell). A single vein forms the *sinus transversus sellæ equinae* described by Haller. A *sinus circularis inferior* is described by Winslow beneath the pituitary body, and formed by branches which take a course nearly parallel to the one usually described. I have found it in six cases only; in twelve others there was a single intercavernous vein beneath the pituitary body.

**Sinus petrosus superior**; *s. petro-basilaris* (Langer); *s. tentorii lateralis* (Weber); *s. petrosus superficialis*.—This sinus runs along the upper border of petrous bone between the layers of the tentorium, from the posterior extremity of the cavernous sinus in front to the lateral sinus behind. It joins the latter at the junction of its horizontal and descending portions. It

varies considerably in size. Its absence was noted in three cases; two of the right side, and one of the left.

A communicating *vas aberrans*, stretching between the ophthalmic vein in front, and the superior petrosal sinus behind, has been described by Verga (*Ann. Univ. di Medic.* 1856). I have found a vessel answering to this description in three instances; they all occurred on the left side.

**Sinus petrosus inferior; s. p. profundus; s. petro-occipitalis superior** (Trolard).—This canal passes from the posterior extremity of the cavernous sinus along the petro-occipital suture to the foramen jugulare. It descends through the anterior compartment of this foramen to join the internal jugular vein. With the purpose of ascertaining the exact level of its termination, I made a careful examination of the bases of eleven skulls after the dissection of the other sinuses had been completed. In eight of the twenty-two sinuses so inspected, the termination was as nearly as possible at the level of the lower margin of the jugular foramen, in nine instances it was a little above, and in the remaining five a little below that level. In two cases it terminated about  $\frac{3}{4}$ ths of an inch below the base of the skull. In those which ended within the foramen, the termination was about the junction of the middle with the inferior thirds (in all cases).

**Sinus petro-occipitalis inferior.**—Under this name Trolard describes an external vein which passes backwards along the petro-occipital suture to terminate in the internal jugular vein.

**Sinus transversus; s. basilaris** (Cruveilhier); *s. fossæ basilaris* (Breschet); *s. basilaris anterior; s. occipitalis anterior; s. o. transversus; Plexus basilaris* (Virchow).—The so-called transverse sinus of our anatomical hand-books ill deserves the name, as it does not form a separate and distinct canal as in the case of the vessels already described. The name of plexus basilaris given to it by Virchow, is much more applicable. It is formed by a network of anastomosing veins, placed between the layers of the process of dura mater which covers the clivus. Some of these open into the inferior petrosal sinus on either side, some communicate anteriorly with the receptaculum or sinus intercavernosus posterior, while others pass downwards to the margin of the foramen magnum to anastomose with the anterior rachidian

veins. I have found no notable variation in their arrangement except in the varying size of the branches which go in the various directions named.

**Sinus occipitalis; s. occipitalis posterior; s. basilaris posterior.**—On account of the contradictory descriptions of this sinus given by different anatomists, I was specially desirous to ascertain what should be looked on as the normal disposition. Accordingly, I noted carefully the arrangement of the occipital venous canals in all the forty-four cases in which I paid special attention to the sinuses. In two of these I could detect no trace of an occipital sinus. In nine cases the sinus was bilateral, one lying beneath the dura mater on either side of the internal occipital crest. In three of these both were very small, and ended below in an anastomosing network at the posterior lip of the margin of foramen magnum, through which a communication was established with the veins of the spinal canal. In two other cases the left was continued on as a *sinus marginalis* to join the lateral sinus at the jugular foramen, while the communication with the veins of the spinal canal was also present. In the other four the continuation of each was to the jugular foramen, while a few very small twigs passed down to join the spinal veins. In thirty-three cases the sinus was single, and occupied the median plane, or nearly so; eighteen of these commenced above at the torcular Herophili; seven opened into the left lateral sinus; five into the right lateral; three communicated with the sinus rectus about a quarter of an inch in front of the torcular. This median sinus descended between the layers of the falx cerebelli till it arrived close to the margin of foramen magnum. In seven cases it was unusually small, and had no further communication than that with the spinal veins. In all the others it split into two, and sent a branch to the jugular foramen along the sulcus marginalis to join the *bulbus venæ jugularis internæ*. In fifteen of these twenty-six the right sinus was somewhat larger than the left, the difference was not, however, very great. In only three cases was the left much larger than the other; in one of these it was about double its bulk. In the others they were as nearly equal as possible.

Of the so-called **emissary veins**, the following peculiarities were noted :—

*Emissarium mastoideum.*—The mastoid foramen for the transmission of a meningeal branch of the occipital artery, and a communicating vein between the occipital veins on the outside, and the descending part of the lateral sinus within the skull—pierces in the majority of cases (as its name implies) the mastoid portion of the temporal bone. In fourteen per cent. of my cases, the foramen was in the masto-occipital suture, and in three instances (out of eighty-eight) the corresponding vessels passed through a hole in the occipital bone itself, close to the suture. In thirty-four of the skulls, the foramen and vein were larger on the right side, in six on the left, in only four instances were they nearly equal. In none of these was it absent on either side, although in a few the size was diminutive; I have, however, seen absence of the foramen in five instances noted from time to time among my other dissections. Three of these occurred on the left side and two on the right.

*Emissarium condyloideum.*—This vessel passes through the canalis condyloideus (posterior condyloid foramen) forming a channel of communication between the lateral sinus, close to its termination, and the upper part of the plexus vertebralis cervicalis (veria jugularis posterior of Cruveilhier). In only thirteen of my forty-four skulls was it present on both sides. In twenty-one cases it was present on the right side, in ten on the left.

*Emissarium parietale.*—This vein passes through the foramen parietale between the superficial veins on the outside, and the sinus longitudinalis superior on the inside. It is accompanied by the ramus parietalis of the occipital artery. Both vessels and foramen are very often absent on one side, but I took no note of the proportion of cases.

*Emissarium occipitale.*—This vessel, which passes out on the external occipital protuberance, and communicates with the venæ diploicæ occipitalis within the substance of the bone, has already been noticed at sufficient length.

A prolongation of the sinus cavernosus into the canalis caroticus has been described by Rektorzik (*pars intra canalem caroticum des sinus caroticus*). It descends, breaking up into a plexus of small veins, which enclose the carotid artery at the lower part of the canal more or less completely, and for some distance below

the base of the skull. They then converge to form one or more trunks which open into the internal jugular vein. I have found in every case one or more small veins taking this course, from the lower part of the sinus cavernosus, so that they differ only in their degree of development, which was very variable.

Nuhn has called the attention of anatomists to a pair of veins which pass through the foramen ovale, communicate with the middle meningeal veins, and after forming a plexus around the commencing portion of the inferior maxillary nerve, terminate in the venous plexus of the infratemporal fossa. The satellite vein of the superior maxillary nerve described by this author has been already mentioned. The veins of the foramen ovale I have found very variable. In eighteen cases I found them to answer to his description on both sides. In six cases there were two veins on the right side and one on the left, in four the condition was reversed; the single vein in every case being nearly equal to the sum of the other two. In eleven instances, there was a single vein on each side. In five cases total absence on one side, three of these being on the right.

A vein or plexus of veins passes through the dense mass of connective tissue which fills the foramen lacrum medium, communicating with the cavernous sinus on the inside, and the external veins below the base of the skull. I have always found this communication when carefully searched for; the size and number of the vessels, however, vary very much.

*Circellus venosus hypoglossi*.—This is the name given by Luschka to the venous plexus which surrounds the hypoglossal nerve as it passes through the inner part of the anterior condyloid foramen. Two veins proceed from this plexus, one of which passes to the plexus vertebralis, the other goes to the sinus petrosus inferior. According to Trolard, they terminate in a vein commencing at the anterior condyloid fossa, to which he has given the name of *confluens condyloideum anterius*. But the truth is, as I have found, that either description may be correct, or a single vein may enter the confluens of Trolard, and the second pass to the inferior petrosal sinus, or plexus vertebralis. All these terminations I have seen, but have not made any note of their proportional frequency.



PRIMARY GROWTH FROM BONE, RESEMBLING IN  
SOME OF ITS FEATURES SCIRRHUS CARCINOMA  
OF THE BREAST. BY GILBERT BARLING, M.B., *Patho-  
logist, General Hospital, Birmingham.*

T. H., aged 41 years, was admitted to the General Hospital, Birmingham, under the care of Mr T. H. Bartleet, by whose kindness I am enabled to describe the case. The patient was a feeble spare man, with an enlargement at the upper part of the sternum about the size of half an orange. The skin was non-adherent to it; the growth was fixed at its deeper part, not hot, pulsatile, nor painful; and the glands above the left clavicle were enlarged. The enlargement was first noticed by the patient fifteen months previously, and it had steadily increased in size, the glandular complication appearing ten months later, and he could assign no cause for the formation. The patient died a fortnight after admission, of double pneumonia. On *post-mortem*, the growth appeared to have originated centrally in the manubrium sterni. It had grown rather more anteriorly than posteriorly; it was distinctly encapsuled, and on section was firm and fibrous; there was no bony matter in it, and the skin was not implicated. The glands were about as large as the end of the thumb, and were rather less firm than the primary growth. Secondary deposits were found on the visceral layer of the left pleura; these varied from the size of a pin's head to that of a pea. The two drawings, for which I am much indebted to Dr Saundby, illustrate two of the most diverse portions of the tumour, there being between these two numerous gradations, in which either the large (as in fig. 1) or the small linear alveolar form (as in fig. 2) predominates. Everywhere the structure is alveolar, except in a few places, as instanced in fig. 1, where two or three cells seem to be imbedded in the matrix; the connective tissue generally is hyaline; the cells are generally retained in their alveoli; they are fairly regular in size and shape, are nucleated, the size of the nuclei varying somewhat. Between the cells when *in situ* it is difficult to recognise any intercellular substance, but at the edges of the tumour, where a few cells are

shed, there appears to be a delicate cementing material of a faintly granular character, but of this last condition I do not feel positive. The vessels run entirely in septa between the alveoli. At the edge of the growth there is a narrow zone sprinkled with small-celled infiltration, the cells staining very deeply with carmine; and in this zone I think I have succeeded in defining the development of the cells of the alveoli, by an increase in the size of the small cells, their double nucleation, and subsequent division generally in a linear direction. Looking at section fig. 1 and not knowing its history, I should have considered it a section of carcinoma such as one finds in the breast, the shape and size of the alveoli, the inability to recognise intercellular substance, and the arrangement of the vessels being very suggestive of such growths; but the appearances shown in fig. 2 are very suggestive of what is described and figured as cylindroma or as plexiform sarcoma. I have therefore abstained from calling this growth either sarcoma or carcinoma, because I think it is open to discussion where such structures should be placed, but my inclination is to define it as an alveolar sarcoma. If carcinoma is necessarily an epithelial growth, this is probably not a carcinoma, because of the seat of its origin, and the cell production from the small-celled infiltration, unless indeed one has a sufficiently robust imagination to assume that, in the development of the sternum, some of the epiblastic layer was included accidentally in the mesoblast, and that from this the growth originated. In favour of its being classed amongst the sarcomata are the encapsulation of the growth, the hyaline nature of its connective tissue, the generally regular shape and size of the cells, and their appearance at times simply imbedded in the connective, from which even in the alveoli they do not readily separate, besides the fact that the immense majority of malignant tumours of bone are sarcomatous, and that it is only in rare instances that there can be any doubt as to their classification.

The glandular affection may be considered as favouring the idea that this is a carcinoma, but I think that the infrequency of glandular complication in sarcomatous growths has been rather exaggerated, for I find that it existed in six cases out of sixty of central sarcomata of bone described by Professor Butlin;

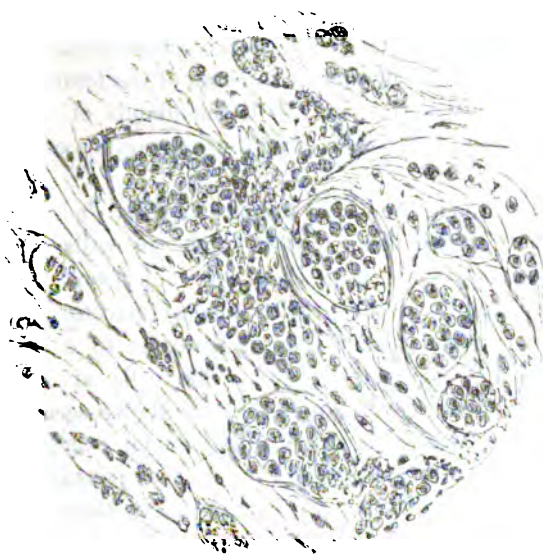


Fig. 1.

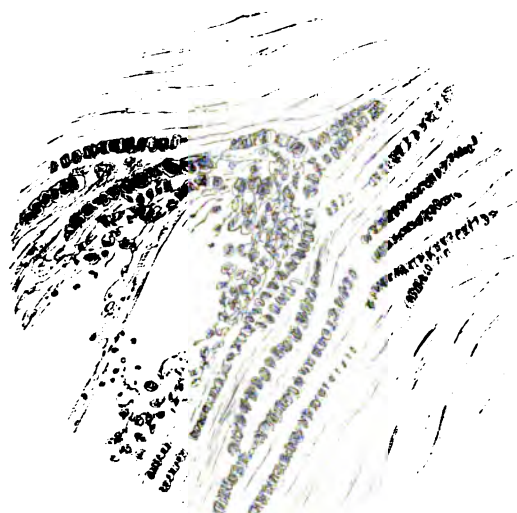


Fig. 2.

and as there were many cases in which no *post-mortem* was made, it is not improbable that glandular affection existed in a greater proportion. A case of central sarcoma of the femur has recently come under my notice, in which the glands of the neck were implicated in a few weeks after the appearance of the primary growth.

Mr Maguire, in the July number of this *Journal*, describes a primary growth from bone which he calls a carcinoma, but it seems to me that there are objections to this ; the general characters of the growth suggest sarcoma, it appears to have been defined rather than infiltrating, to contain cavities and old blood pigment deposits. The cells are described as epithelial ; but in the drawings they are shown as nearly regular in shape and size, with small single nuclei, except where they are developing from giant cells ; indeed they resemble the cells of a large round celled sarcoma much more than the cells one expects to find in a carcinoma concerning which there can be no debate. Again, the development of the cells from the spindle and round cells of the stroma and from the myeloid cells, remove all possibility of the growth being in any sense epithelial, and suggest very readily the development of a sarcoma. As to the absence of embryonic tissue in the walls of the vessels, I think this may be explained by the fact that the alveoli are not of sufficient size to require vessels to pass through them for the nutrition of the cells, and hence the vessels are confined to the stroma, and so escape the cell part of the growth,—a condition very different from that where the vessels are found imbedded in the midst of a large collection of embryonic tissue.

I have offered these objections, because, if any good is to come of our classification, it will be principally in the direction of prognosis ; and the description of this tumour induces me to think that its affinities are to myeloid sarcoma rather than to a carcinoma, and this view of a similar growth after operation would certainly justify a better prognosis than that supported by Mr Maguire. In this case, however, the history is too brief to give one any idea of its malignancy from a clinical point of view.

ON THE REPRODUCTION OF THE "FEELER" OF  
THE LOBSTER'S ANTENNA. By GEORGE BOND  
HOWES, *Demonstrator of Biology in the Royal School of  
Mines.*

THE methods of reproduction of lost parts among the *Arthropoda* and the variations and monstrosities connected therewith, are well known; but the undermentioned example, which has come under my notice, although at first sight a monstrosity, or at least a sport, appears upon further consideration to be one of an occurrence normal to at least the lobster.

The specimen was about two-thirds grown (sex undetermined), having all its appendages, including the left antenna, uninjured, while the "feeler" of that organ of the right side had the appearance represented in fig. 1, and consisted of a rounded filament in a close-set spiral of six coils, the apex of which looked downwards and inwards, so that it came under cover of the surrounding parts. Examined under a hand lens this structure exhibited features common to the multi-articulate "feeler," being in reality a tapering filament—*en*—and showing the characteristic annulations, which were just appearing along its whole length. The entire organ at this stage was of a delicate nature, as the cuticle covering it was still uncalcified and whitish in colour.

That this is the typical method of reproduction of the "feeler" in the lobster, I have no doubt, although I have failed to obtain any stage intermediate between this one and the completely restored appendage, and the following considerations would appear to justify such a conclusion.

The brittle nature of the organ is patent, and in warfare the general tendency is for it to snap across when seized, rather than give way at its base, which is its strongest part, and the examination of such feeler-wanting specimens as reach our laboratories, shows, from the nature of the wound, that the loss is in the majority of cases due to the usage they get at the hands of the dealers and others through which they pass; hence

it is only rarely that the conditions before mentioned are met with, still one occasionally finds an antenna the "feeler" of which exhibits along its length a decided *twist* (permanent), such as is shown in fig. 2. As the fully-developed structure is a flattened one, this can be seen by following its edge. That the appendage is primarily developed in the customary straight fashion, the description and exquisite figures of Sars<sup>1</sup> leave no doubt, and as this condition is persistent throughout life, I am disposed to consider the aforesaid twist to be the expression of the last uncoiling of the spiral described.

The only other record of a like example known to me, is one by Krøyer,<sup>2</sup> for the Penæid *Sergestes*; here, however, the coils were more numerous, and came completely under cover of the enlarged exopodite ("squame") of that genus. Writing in 1855, he may be perhaps excused the asking "Is it a parasite?"

The importance to the animal of this appendage is obvious; and Newport<sup>3</sup> describing the reproduction of the same in the Millepede *Julus*, lays great stress upon the "careful preservation" of the young limb "from contact with anything." Granted this necessity in the lobster, the method evolved to bring about the safety of the germ is as unique as it is effectual, for the thing in life came completely under the protection of even the large Maxillipedes. Measurement shows that the depth of the spiral is (fig. 1 *a*) 8 millim., while that of the extended "feeler" thus coiled would be 65 millim. *b*, and nearly one-third of the length of its uninjured fellow.

Obviously, then, it becomes a matter of great importance to a creature of so hostile a disposition to ensure the safety of this delicate germ of so important an organ. I regret to say that some crayfish, in which I had removed the antenna hoping the same might occur, died off before the moulting season came round.

<sup>1</sup> *Om Hummerens postembryonale udvikling*, G. O. Sars, Christiana, 1874. See also "The Early Stages of the American Lobster," S. J. Smith (*Trans. Connecticut Acad. of Arts and Sciences*, vol. ii. part 2, 1873).

<sup>2</sup> *Forsøg til en Monogr. fremstill. af Kraebsdyrsl Sergestes (S. Edwardsi)*, H. Krøyer, 1855.

<sup>3</sup> "On the Reproduction of Lost Parts in Myriapoda and Insecta" (*Newport Phil. Trans.* 1844).

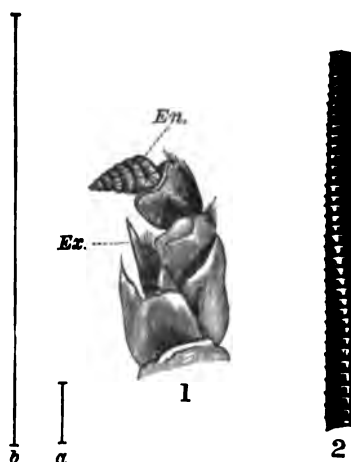


Fig. 1, The right antenna, showing the reproducing "feeler" (endopodite) En. Ex. the squame (exopodite). Slightly enlarged.

a. Actual depth of the spiral.

b. Length of the coiled filament.

Fig. 2. Portion of a fully reproduced "feeler," showing its spiral twist still evident. Reduced to half natural size.

THE BOSTON  
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HISTORY

ON THE COMPARATIVE ANATOMY OF THE LYMPHATICS OF THE UTERUS. By GEORGE HOGGAN, M.B. (Edin.), and FRANCES ELIZABETH HOGGAN, M.D. (Zurich), M.K.Q.C.P. Ireland. (PLATES I. and II.).

ALTHOUGH nearly two hundred years have elapsed since Nuck first observed the lymphatics on the peritoneal surface of the pregnant uterus of some of the lower animals, and the same vessels were afterwards studied by Méry, Mascagni, Cruickshank, Winslow, and other later anatomists upon the pregnant or puerperal uterus of woman, yet the whole history of the intimate or microscopical investigation into the lymphatics of the normal uterus might be comprised within the last twenty years. During that short period no other organ has perhaps been made the subject of so many special researches with regard to its lymphatics; and yet the most important part of these are not only unknown, and their existence denied, but even as regards the remainder the greatest ignorance or misconception exists. Nevertheless, perhaps no organ is more exposed to injury and denudation of its internal surface and, consequently, to the dangers following morbid absorption from that denuded surface than the uterus. Nowhere, therefore, is it more important that the relation which the lymphatics bear to that surface should be intimately and completely known. The disasters so often following parturition, clearly shown to be due to morbid infection, and to which nearly one-half of the human race are exposed at some period or other of their lives, make our ignorance of the channels of absorption from the interior of the organ a scandal to obstetrics in particular, and to scientific medicine in general.

In our English text-books the compilers appear to have derived all their information upon the present subject from no more modern an authority than Mascagni. Even special writers seem, with one or two exceptions, to know only of the so-called subserous lymphatics, a division which we shall hereafter show to belong, both anatomically and physiologically, to the musculature. If we go back as far as the time of Cruickshank



we are informed by that observer (*Anatomy of Absorbent Vessels*, p. 143): "In the gravid uterus (*i.e.*, human) the trunks of the absorbents are large as a crow-quill, and the vessels so numerous that when they only were injected with quicksilver one would have been tempted to suppose that the uterus consisted of absorbents only. In the unimpregnated uterus they are not so easily detected, but when the viscus has been injected by the arteries and veins, and has afterwards been macerated in water by putrefaction, air gets into the absorbent vessels and makes them perfectly distinct."

Coming down to our own day, we find that Kölliker mentions only these subserous lymphatics, with the remark that "the lymphatics which *probably* begin in the mucosa are numerous, and form coarse and fine networks under the peritoneal surface."

Chrobak, who writes the article on the Uterus in Stricker's well-known *Handbook*, in 1871, frankly confesses that "the lymphatics in the interior of the uterus are as good as unknown."

Sappey, a veteran practical investigator of the lymphatic system, states in his *Anatomie Descriptive*, in 1869, p. 761: "The lymphatic vessels of the vesical and uterine mucosa have not yet been injected;" and again, p. 826: "The lymphatics of the uterus arise from the musculature of the organ; *probably* some come out of the uterine mucosa, but on this last point observation has established nothing up to the present time." Later still, however, in the third edition of his *Anatomie Descriptive*, published in 1876, p. 801, Sappey, speaking of the smooth mucous surfaces which are destitute of lymphatics, says: "With the foregoing mucous surfaces we may include the mucosa of the body of the uterus, and that of the Fallopian tubes, upon which no one has ever been able to discover any trace of lymphatics. But in studying the vesical and urethral mucosa, by the light of the unsuccessful character of all the investigations that have hitherto been made at different parts of their course, we find them differing from those of the uterus and tubes in the conclusions to be drawn from this want of success. We must, in fact, consider that the latter are not so easily adapted for this kind of investigation as the mucosa of the urinary

apparatus, that they have been very much less explored than these, and that the lymphatics arise in great numbers from the body of the uterus. Now, there is nothing to prove that they do not come, in part at least, from the sides of its cavity; that is to say, from the mucosa which lines it. Far from denying their existence in the thickness of that mucosa, I believe, on the contrary, that it is very probable. Thus, while the latest scientific researches appear to promise nothing, on the other hand they permit us to hope for better results."

In 1867 an article appeared on the structure of the uterus by Hjalmar Lindgren, which deserves especial notice, for several reasons. It is written in Swedish, and published in the *Medicinskt Archiv* of the Carolinska Institute of Stockholm, and although apparently well known to the Germans, not a single copy seems to exist in London, where all the principal and scientific libraries were ransacked for it in vain. It was only by the kindness and careful searching of our friend, the late Professor Georgii, at Stockholm, that we were able to procure a copy of the article.<sup>1</sup> To the whole history and question of the lymphatics Lindgren devotes scarcely two pages, but he gives drawings of lymphatics, most of which are apparently incorrect, one of which, however, bears the stamp of truth, and deserves greater consideration than Lindgren has bestowed upon it. This is to be found in the centre portion of fig. 1, Plate IV., which represents what are virtually only blue casts of small portions of the lymphatics of the mucosa of the neck of the uterus; but as neither cells, vessels, glands, nor other tissues are represented, one is left to guess the relationship these lymphatics bore to the tissues in which they are supposed to lie. And yet we believe that little portion partly to bear out Lindgren's claim that they are portions of the lymphatics of the mucosa of the neck, as stated in his modest conclusion, p. 36: "On the lining mucous membrane the lymphatics extend themselves principally upon the mucous membrane of the neck of the womb. I have not succeeded in making a true injection of the lymphatics of the body of the womb." Unfortunately for Lindgren's claim, he gives in fig. 2 of the same plate a much enlarged drawing of two

<sup>1</sup> This copy has now been deposited by us in the library of the Royal College of Surgeons of England.

of these injected lymphatics, with their relation to the branched connective tissue cells of the locality, and shows in this drawing the blue injection passing from the cavities of the supposed lymphatics along the cavities in which the branched cells of the connective tissue are supposed to lie, as bearing out the theory that the origins of these lymphatics are in these cavities. There is no doubt that this drawing is copied correctly from the original, but it shows also in the clearest manner possible that the whole injection there is a mistake—an artificial production which can easily be repeated and recognised by a method already explained by us in this Journal.

In short, it is evident that at one part Lindgren has hit a lymphatic, but he has so far missed his good fortune as afterwards to offer an artificial production as his typical example, under high power, of a lymphatic in its relations to the branched cells of the part, while of its relations to other tissues, or even its precise locality, he says nothing. It is evidently such considerations which cause Leopold, in his special article written five years afterwards, to reject entirely all Lindgren's conclusions and figures, although Frey, in his *Histology*, 1870 edition, accepts Lindgren's conclusions as far as regards the neck of the uterus, adding (p. 540): "While for the lymphatics of the mucosa of the body of the uterus further investigations are necessary." These further investigations were shortly afterwards undertaken by Leopold, who also (apparently for the first time) investigated the course of the lymphatics of the musculature of the organ. His article,<sup>1</sup> as the last and most complete one on the general lymphatic system in the normal uterus, will be specially and constantly referred to by us; but as we disagree with by far the greater part of his views, we shall merely quote for the present his conclusions on the lymphatics of the mucosa, which are remarkable as coming after those of Lindgren, and with the results of that investigator before him. At p. 31 he gives the following conclusion strongly emphasized:—"The results of my investigations are as follow: IN THE MUCOSA OF THE UTERUS OF THE AFORE-MENTIONED ANIMALS (*i.e.*, sheep, swine, rabbits, &c.) AND OF WOMEN, THE LYMPHATICS ARE

<sup>1</sup> "Die Lymphgefäße des normalen, nicht schwangeren Uterus," im *Archiv für Gynäkologie*, B. 6, für 1874, p. 1.

NOT, AS IN THE SUBSEROSEA AND MUSCULARIS, TRUE ROUND VESSELS, BUT IN THE MUCOSA THERE IS A COLOSSAL SYSTEM OF CAVITIES OR HOLLOW SPACES (LYMPH-SPACES), WHICH EXTEND FROM THE EPITHELIAL LINING OF THE UTERUS TO THE LYMPHATICS OF THE MUSCULARIS, THE FRAME-WORK OF THE MUCOSA BEING FORMED BY BLOOD-VESSELS, GLANDS, AND CONNECTIVE TISSUE ELEMENTS. Or, in other words, *the uterine mucosa is to be considered as a spread-out lymph gland, permeated by glands and blood-vessels—a superficial covering of lymphatic gland tissue, which possesses consequently no special lymphatic vessels, but consists of hollow cavities (lymph-sinuses) lined by an endothelium.*" We shall afterwards explain but not repeat the above remarkable conclusion.

About and before this time appeared two other articles, which we mention only to show that we have not forgotten them. One of them is a work in Russian,<sup>1</sup> by Fridolin, which we have failed to obtain, as it does not seem to exist in any scientific library in London, all of which have been searched for it. We are therefore forced to rely on a reviewer's (Schwalbe's) abstract of it, which states that "Fridolin has not specially followed the lymphatics into the mucosa; nevertheless, he has made this much certain, that the lymphatics which come upon the surface (*i.e.*, peritoneal) from the deeper structures, have their special origin in the mucosa." Well may Leopold ask with amazement how so certain a conclusion could be found if he had never followed the lymphatics into the mucosa.

The second work we referred to is by Dr Lucas Championnière.<sup>2</sup> These writings have obtained much prominence, both in France and in one of the ablest English text-books on Obstetric Anatomy, but for what reason it is difficult to say. In his earlier writings the author specially gives his attention to Lymphangitis, and grafts upon this some speculative opinions about the lymphatics. Thus he states, at p. 12: "All the lymphatics begin equally in the muscular tissue and the mucous surface. *Although here direct demonstration is imperfect, we have a right to expect, from the development of these vessels parallel to*

<sup>1</sup> "On the Lymphatics of the Pregnant Uterus," *Journal of Military Surgery*, St Petersburg, 115 Division, part ii. p. 105.

<sup>2</sup> *Lymphatiques Uterines et Lymphangite Uterine*, Paris, 1870; *Les Lymphatiques Uterines et leur rôle*, Paris, 1875.

*those of the placenta, their presence and their abundance in the mucosa."*

The easily-written sentences which we have quoted from the two last-named authors are samples of the guesses by which non-investigating writers continually seek to discount the future ascertained facts of investigators. From the text and drawings of Dr Championnière's work it appears that his field of investigation was the superficial or subperitoneal and pus-filled lymphatics of half-contracted uteri of fatal parturitions. Some of these he endeavoured, but failed, to fill completely with quick-silver, and these partial puro-mercurial distended localities furnished him with the above information.

In his later brochure, written after the publication of Leopold's article, Dr Championnière takes credit for certain of his predictions, which Leopold had verified, and continues to form elaborate deductions from the presence of stomata and similar mythical structures.

Finally, we have to direct attention to the last special research on our present subject, an article by Dr Vladislas Mierzejewski on the "Subserous Lymphatics of the Uterus."<sup>1</sup> This little research has been apparently conducted principally on the normal uterus of the sheep, and is illustrated by a plate, which appears to be perfectly correct and natural. The writer specially draws attention (p. 216) "to the extreme richness of the subserous portion of the uterus in lymphatic vessels." This, although partially true of the sheep, would give a very incorrect idea in relation to comparative anatomy, for in the mare not one lymphatic is to be seen in that locality, and in most animals the supply is extremely irregular, plentiful at one place, and perhaps altogether absent from the rest of the subserous surface. To this, however, we shall afterwards recur, remarking merely in the meantime that Mierzejewski's drawings are more acceptable to us than his conclusions, inasmuch that we feel inclined to appeal to the former to disprove the latter.

Of the foregoing historical sketch we have now to remark that all the conclusions were obtained by injecting, or attempting to inject, the lymphatics with mercurial or coloured injection masses. Leopold, in one case, gives a drawing from an injection

<sup>1</sup> *Journal de l'Anatomie*, 1879, p. 201.

of silver solution ; and both he and Mierzejewski show lymphatic endothelium treated by silver. None, however, show distinct lymphatic vessels obtained by imbibition of silver solution ; and in certain cases the latter repudiates the process. We, however, have obtained nineteen-twentieths of our results by imbibition with salts of silver. Specimens thus prepared gave fairly satisfactory results, as those who have seen our preparations and the photographs taken directly from those preparations under the microscope, when exhibited at the Obstetrical Society on the 12th of January 1881, will be able to testify.

According to our hitherto almost invariable custom, we took the opportunity of there submitting the drawings which illustrate this research, for comparison with the preparations from which they had been taken, along with a very short account of the progress already made. At that time our studies were so far incomplete that we had not had an opportunity of applying our methods to the still living tissues of the human uterus. We had indeed been able to verify by modified injection the similarity existing between the lymphatics of the mucosa of the human uterus with those of the uteri of some of the lower animals, and more especially with that of the mare ; but we had not been able to obtain a human uterus so shortly after death that we could make a superficial preparation of the mucosa by the method of silver imbibition, which would show the exact relations of the lymphatics with the blood-vessels, glands, and general cell-elements of the locality. It was for this reason that we only gave a short *résumé* of the progress we had made, reserving ourselves for a future occasion, when we should be able to include the results of our methods on the human uterus in our more complete account. In the meantime, many of our obstetrical brethren promised to procure for us, if occasion offered, a fresh human uterus ; but as more than six months have elapsed since then without any such occasion offering itself, we deem it advisable to present our interim account, even with the incompleteness referred to, accompanied by the present explanation of the cause of it.

Hitherto it has been the practice in treating of the lymphatics of the uterus to separate these, for the sake of convenience, hypothetically into three divisions, corresponding to the anatomical

structure of the uterus. Thus we have had those, 1st, of the subserosa ; 2d, of the muscularis ; 3d, of the mucosa. We have already intimated that the first belong, both anatomically and physiologically, to the lymphatics of the outer surface of the longitudinal layer of muscle, and not to the subserous tissue. Consequently we have to divide these lymphatics into only two divisions: 1st, those of the muscularis ; 2d, those of the mucosa ; leaving altogether out of consideration those of the subserosa as a separate division. Before directing special attention to either, it may be advisable that we should first give a description of the *technique* employed by us in the preparation of the various tissues of the organ in the different classes of the Mammalia to which we have given our attention.

We have stated already that to our knowledge no investigation into the lymphatics of the uterus had been made as yet by silver imbibition. It was consequently our first object, in endeavouring to utilise that method, to devise a suitable *technique* for our purpose ; and, after many trials, we arrived at a very simple one, which can be explained in few words. As in the great majority of the Mammalia, the uterus is formed of very distinct layers of different tissues, it was evident that the arrangement of the lymphatics would almost certainly be in planes parallel to the layers or surfaces ; and we considered that they ought to be specially investigated as such, a course which had not hitherto been employed, only transverse sections having been studied, in addition to the appearances given by the injections on the outer and inner surfaces of the organ. Stated generally, our plan was as follows :—In the case of all animals, whether of the size of a mare or of a mouse, the uterus having been extracted was laid open alternately, either along its attached or free border, and the tissue distended as a membranous sheet. When the uterine wall was thin enough to be transparent, it was at once mounted on our histological rings, and the surface of the mucosa denuded of its epithelium and mucus by shaving it with a sharp scalpel to a greater or less depth, according to the animal class which it belonged to. This having been done, a one per cent. solution of nitrate of silver was quickly poured upon, and as quickly poured off, one or both sides of the tissue, as considered advisable. After a few minutes' exposure to a dull

light this was carefully washed, and a one per cent. solution of chloride of gold allowed to remain in contact with the washed surface or surfaces for a couple of minutes. Then the membrane was washed and exposed to light until the desired reaction had taken place. This would be verified by continual examination under the microscope of the tambourine membrane thus formed. If successful, it could be clarified in the usual way with alcohol and oil of cloves, the disc excised, and mounted as a permanent preparation.

In addition, however, to the foregoing general directions, almost every class or size of animal requires special precautions or manipulations, for there are the widest possible differences in the arrangement of the tissues in the different classes of animals, and some of these must be mentioned *seriatim*. Let us take for example the uterus of a mare or ass, with a wall too thick to be transparent. In such a case the uterus, having been laid open, is tightly stretched across a wooden board by nails inserted at the edges of the sheet. With a very sharp-pointed scalpel the mucosa can be easily dissected off the musculature, some little care being taken not to cause undue distension or injury to it while dissecting it off. If this is skilfully done it will be found unnecessary to cut the surface, but one may simply snip across with the point the vessels which here and there pass from the musculature to the mucosa, and attach the two together. When a sufficiently large sheet of the mucosa has been dissected off (being grasped by the fingers merely at the edges without touching the surface of the centre of the sheet) it is gently laid upon the lower of the pair of histological rings (which may be 2 inches in diameter), and the upper ring is gently applied and forced upon the lower, taking care not to tear the delicate membrane, the epithelial surface of which must be uppermost, which is thus stretched like the head of a tambourine upon it. The mounted portion may now be severed from the rest of the sheet.

It is to be particularly observed, in the present case of the chevaline race, to scrape the epithelial surface of the mucosa with the utmost care and lightness, only apparently sufficiently to get rid of the mucus lying upon the surface, for the lymphatic vessels lie and ramify so closely beneath the lining epithelium that the cells of that one layer can scarcely be removed suffi-



ciently to allow the silver solution to penetrate more deeply, without injuring at the same time the crenated cells forming the endothelial wall of the more superficial lymphatics. The shape and arrangement of the glands, lymphatics, blood-vessels, &c., of the mare are widely different from those of most other animals, and approach nearer to those of the human uterus than those of any Mammalian class lower than the monkey. For that reason we shall give more prominence to them in the following pages.

When the epithelium and mucus of the outer surface has thus been lightly and carefully removed, the silver and gold solutions are to be equally applied to both surfaces of the tambourine membrane, and thus a complete and beautiful demonstration of the lymphatics, as they appear differentially on the two surfaces, may very readily be obtained. Of these drawings are given in figs. 1 and 2, both being drawn under the same low power of the microscope. Without going at present into a minute description of the character and relations of these lymphatics, we may add that while the same methods of preparation are applied to the uteri of cows, sheep, and goats, the scraping or shaving of the surface of the mucosa must be very deep and effectual in them before we can arrive at the more superficial lymphatics of the mucosa; in other words, the most opposite treatment, as far as removal of the surface is concerned, must be applied respectively to the mucosa of such animals as the goat and the mare.

The mucosa having been carefully dissected off and disposed of as described, the remaining portion of the uterine wall may be unnailed and treated in the same way, when a separate arrangement or layer of lymphatics will be found lying on the mucosa side of the musculature, and the subserous surface may be equally prepared in portions of the same uterine wall, but in the mare no lymphatics will be demonstrated within the subserosa. In other cases it will be found possible and advisable to dissect the inner circular layer off the outer longitudinal layer of muscle, and to treat the respective surfaces with silver, &c., which will complete the demonstration of the whole of the lymphatics of the organ.

When the uteri of smaller mammals are to be examined, other precautions are necessary, more especially as the musculature is apt to contract to such an extent on exposure to the air

that, without special manipulation, it is impossible to distend the uterine wall on the histological rings. In the case of the mouse, whose unimpregnated uterus is so delicate and small that it cannot well be placed on rings at all, it will generally be found advisable to distend the organ with air by means of an injecting-nozzle or canula fastened into the vagina. When thus distended and ligatured at the neck and Fallopian tubes, the organ may be treated externally with the solutions of silver and gold, and clarified with glycerine before being opened into. This, however, will at most only show the arrangements of the outer portion of the organ. For the inner surface it had better be laid upon a thin dry sheet of cork, and the uterine tube having been laid open by a pair of fine-pointed forceps is spread out carefully upon the cork, to which the peritoneal surface adheres sufficiently to allow the mucous surface to be denuded, treated with the solution, and fixed and clarified in glycerine. For the pregnant uterus of the mouse at term, and for the unimpregnated uteri of rabbits, guinea pigs, &c., other precautions are necessary; and the following plan is of very general and satisfactory application. In a slab of cork (a piece of an ordinary cork sole is what we always use) circular holes are cut that admit of the passage of the respective sized histological rings through them. Choosing a ring of a suitable size, the portion of pregnant uterus containing one foetus is laid across the corresponding sized orifice in the cork slab, and fixed at each end by small pins. With fine pointed scissors a small incision is made into the distended bag, at each extremity, which is then pinned out on either side of the originally fixed pins, and in this way, by cutting and pinning a small portion alternately, the whole uterine wall may be stretched across the hole in the cork slab. The lower ring may now be applied from below the slab upon the stretched out tissue, the upper ring fixed upon it, the pins taken out, and the whole tambourine removed by pushing it up through the hole in the cork, when it can be treated as before described. This use of the cork slab is very convenient, as it dispenses with the help of an assistant, which is often necessary to keep a contractile membrane distended upon the lower ring while the upper ring is being affixed to it. With the application of the various modified manipulations we have enumerated, the various tissues

of the organ in question may be prepared in almost all classes of animals.

Sometimes, instead of the circular histological rings we have so often referred to, we use narrow oblong arrangements of the same nature, with corresponding oblong holes in the slab of cork. These are particularly suitable for the narrow elongated uteri of rats, rabbits, &c. We have recommended that the organ should be sometimes opened out along its attached border, sometimes along the free border. This is advisable from the fact that there is often a great difference between the arrangements of the glands, lymphatics, &c., at these opposite points on the tube, due apparently to the fact that the placenta in some of the lower animals without cotyledons is attached as a rule against the attached border only.

In order to understand the comparative general arrangement of the lymphatics of the organ, we must begin our study with the simplest forms in the smallest mammals, and pass gradually to the more complex forms in the larger mammals, and to the more minute arrangements in the special tissues and layers. Beginning, therefore, in the mouse, and passing backwards against the lymphatic stream, we find one or two large efferent lymphatic trunks accompanying the main artery and vein within the broad ligament, lying nearly parallel to the respective horn of the uterus, or approaching gradually nearer to it at its extremity. From this main lymphatic branches pass off nearly at right angles, generally accompanying corresponding branches of the artery or vein passing directly to the uterus. As soon as these lymphatics reach the uterine tube, they (unlike their behaviour on the intestinal tube) at once pierce the outer longitudinal layer of muscle, and divide into two or three main branches, which pass respectively on either side of the tube, and after running parallel to the fibres of the inner or circular muscular coat, but at right angles to the fibres of the outer layer, they finally anastomose at the unattached border of the uterine tube. While coursing circularly round the tube, between the muscular layers, many branches are given off nearly at right angles to the main vessels which ramify between the layers. Other branches pierce the longitudinal outer layer, and after a short course on the subserous aspect of that layer, again pass

apparatus, that they have been very much less explored than these, and that the lymphatics arise in great numbers from the body of the uterus. Now, there is nothing to prove that they do not come, in part at least, from the sides of its cavity; that is to say, from the mucosa which lines it. Far from denying their existence in the thickness of that mucosa, I believe, on the contrary, that it is very probable. Thus, while the latest scientific researches appear to promise nothing, on the other hand they permit us to hope for better results."

In 1867 an article appeared on the structure of the uterus by Hjalmar Lindgren, which deserves especial notice, for several reasons. It is written in Swedish, and published in the *Medicinskt Archiv* of the Carolinska Institute of Stockholm, and although apparently well known to the Germans, not a single copy seems to exist in London, where all the principal and scientific libraries were ransacked for it in vain. It was only by the kindness and careful searching of our friend, the late Professor Georgii, at Stockholm, that we were able to procure a copy of the article.<sup>1</sup> To the whole history and question of the lymphatics Lindgren devotes scarcely two pages, but he gives drawings of lymphatics, most of which are apparently incorrect, one of which, however, bears the stamp of truth, and deserves greater consideration than Lindgren has bestowed upon it. This is to be found in the centre portion of fig. 1, Plate IV., which represents what are virtually only blue casts of small portions of the lymphatics of the mucosa of the neck of the uterus; but as neither cells, vessels, glands, nor other tissues are represented, one is left to guess the relationship these lymphatics bore to the tissues in which they are supposed to lie. And yet we believe that little portion partly to bear out Lindgren's claim that they are portions of the lymphatics of the mucosa of the neck, as stated in his modest conclusion, p. 36: "On the lining mucous membrane the lymphatics extend themselves principally upon the mucous membrane of the neck of the womb. I have not succeeded in making a true injection of the lymphatics of the body of the womb." Unfortunately for Lindgren's claim, he gives in fig. 2 of the same plate a much enlarged drawing of two

<sup>1</sup> This copy has now been deposited by us in the library of the Royal College of Surgeons of England.

of these injected lymphatics, with their relation to the branched connective tissue cells of the locality, and shows in this drawing the blue injection passing from the cavities of the supposed lymphatics along the cavities in which the branched cells of the connective tissue are supposed to lie, as bearing out the theory that the origins of these lymphatics are in these cavities. There is no doubt that this drawing is copied correctly from the original, but it shows also in the clearest manner possible that the whole injection there is a mistake—an artificial production which can easily be repeated and recognised by a method already explained by us in this Journal.

In short, it is evident that at one part Lindgren has hit a lymphatic, but he has so far missed his good fortune as afterwards to offer an artificial production as his typical example, under high power, of a lymphatic in its relations to the branched cells of the part, while of its relations to other tissues, or even its precise locality, he says nothing. It is evidently such considerations which cause Leopold, in his special article written five years afterwards, to reject entirely all Lindgren's conclusions and figures, although Frey, in his *Histology*, 1870 edition, accepts Lindgren's conclusions as far as regards the neck of the uterus, adding (p. 540): "While for the lymphatics of the mucosa of the body of the uterus further investigations are necessary." These further investigations were shortly afterwards undertaken by Leopold, who also (apparently for the first time) investigated the course of the lymphatics of the musculature of the organ. His article,<sup>1</sup> as the last and most complete one on the general lymphatic system in the normal uterus, will be specially and constantly referred to by us; but as we disagree with by far the greater part of his views, we shall merely quote for the present his conclusions on the lymphatics of the mucosa, which are remarkable as coming after those of Lindgren, and with the results of that investigator before him. At p. 31 he gives the following conclusion strongly emphasized:—"The results of my investigations are as follow: IN THE MUCOSA OF THE UTERUS OF THE AFORE-MENTIONED ANIMALS (i.e., sheep, swine, rabbits, &c.) AND OF WOMEN, THE LYMPHATICS ARE

<sup>1</sup> "Die Lymphgefäße des normalen, nicht schwangeren Uterus," im *Archiv für Gynäkologie*, B. 6, für 1874, p. 1.

These longitudinal sinuses or ditches are the true collecting lymphatics of the organ, and the increase they have undergone, both in calibre and length, is perhaps the best marked change caused by pregnancy in any part of the lymphatic system of the organ. These sinuses are similar in position and function to the parallel drains dug along the slopes of a tilled field, joining, at right angles, the main drain which lies at the lowest side of the field, that main drain being represented in the uterus by the huge valved efferent lymphatics which lie one on each side of the main arterial and venous trunks which encircle the organ, between the muscular layers.

Notwithstanding the great increase in the size of these lymphatics, there is no addition of any strengthening elements to their walls. Both in the pregnant and non-pregnant states, these walls consist only of the one layer of crenated endothelium, without any muscle fibres whatsoever. In the pregnant mouse, the outer muscular layer is so thin that these collecting and efferent lymphatics can often be made evident from the outer side of the longitudinal layer, if the serous endothelium have been previously removed in order to permit the silver solution to penetrate through the layer and make the lymphatics evident. When we reach an animal the size of the pregnant guinea pig, this is no longer possible, but here we observe great lozenge-shaped gaps between the bundles of muscle forming the outer longitudinal layer. In many cases, the whole of the floor of such gaps appears to be filled by a lymphatic vessel large enough to occupy the whole field of the microscope under a power of fifty diameters. In fact, the gap seems paved with crenated lymphatic endothelium, which is really the wall of an intermuscular lymphatic, made evident in consequence of the separation of the special muscular bundles in the longitudinal layer caused by the great distension undergone during the progress of pregnancy.

It may give us an idea of the physiological importance, as a whole, of the uterine lymphatics, to compare them with the lymphatics of similar hollow organs in the same animal, as, for example, the ileum, which in general appearance and anatomical structure (barring the villi) much resembles the uterine cornua. Tracing, in the case of the ileum, the efferent lymphatics of the mesentary backwards, we find that these, on reaching the intes-

tine, divide into two or more large branches, which, passing up equally on either side of the intestine, *but lying as great streams in the subserosa*, there, after spreading like a tree, divide into numerous branches which pierce both layers of muscle, to join the colossal system of comparatively less valvular lymphatic vessels lying in the mucosa. Between the two layers of muscle, we have no lymphatic arrangement comparable to that in the uterus, for we find there only anastomosing vessels between the perforating branches. It is, however, the comparatively speaking regularly arranged plexus of colossal lymphatics forming small meshes, which lies in the mucosa, having cul-de-sac prolongations extending into the villi on the one surface, and joined by the efferent branches on the other, which forms the chief feature of the intestinal lymphatics, and make it clear that the adaptation of the lymphatics in that organ is directed towards one surface, from which absorption of the nutrient material of the chyme takes place.

As we have no such arrangement in the muscular tube of the uterus, we feel bound to consider that there the lymphatics are only adapted to the periphery of a bag, having, it is true, minute orifices, above and below, more or less closed by the nature of the organ. In the pregnant uterus, each foetus indeed seems to form its own bag, like spherical sausages in a string, without distinct pervious communication between them. In short, in the intestinal tube, the lymphatics are directed to the most minute points of the internal surface; in the uterine tube, they are not directed to points on either surface, but adapted generally to its periphery, regarded as that of a tube or bag. As we ascend the scale in size of animals, we find considerable modifications in arrangement and by the formation of additional layers of lymphatics, but nevertheless the same physiological plan that we have just described in the case of the mouse, is traceable throughout the series of mammals.

In the uteri of middle-sized animals, such as sheep and swine, the intermuscular layer still remains the principal one, and the branches, passing through and ramifying upon the outer muscular layer, become much more numerous and more complex in their arrangements, and more evidently the special lymphatics of the muscle and not the lymphatics

of the subserosa. That intermuscular layer, now, however, sends great offshoots through the inner circular layer of muscle, and these offshoots not only pass round the mucosa surface of the inner circular layer of muscle, in streams parallel to its fibres, but give off, in turn, great branches, which pass on to the deeper or muscular surface of the mucosa. These again give off loops extending as convex bows towards the epithelial surface or cavity of the uterus. In the pig, these branches of the submucosa run comparatively straight, and still parallel to the circular muscle fibre of the inner layer, but in the sheep, goat, mare, &c., we have a complete system of continually anastomosing vessels, forming junctions with each other at almost every valvular dilatation, as seen at figs. 1 and 8, which show the plexus of lymphatics as a layer on the plane of the deep surface of the mucosa.

Here and there the branches seem to stop short with a greatly dilated circular end. This, however, merely marks the point where the lymphatic passes into the substance of the mucosa, going towards its epithelial or free surface, the increased circular enlargement being caused by the penetrating link (if we may so speak of the short intervalvular portions of lymphatic) passing off, at right angles to the general plane of the deep surface plexus; and being seen flattened or compressed in the direction of its axis, it thus presents the appearance seen at the extremities of the vessels in figs. 1 and 8. The forms taken by the ultimate superficial loops of the plexus in the mucosa is particularly well shown in figs. 6 and 7, from the sheep. These loops lie with their convexity towards the free epithelial surface amongst the coils of uterine glands, to the greater part of which they lie superficial, as seen in fig. 6. In the centre of the arc, however, the lymphatic dilates into a wide sinus where the vessel lies nearest to the surface. In other words, we have a number of dilated lymphatics forming sinuses that are connected with each other by comparatively narrow or slender lymphatics, which also establish anastomoses with the deeper and larger lymphatics of the mucosa.

In none of the middle-sized animals we have been considering does the lymphatic system obtain the complete development, as a surface structure, that it afterwards does in the mare, for, as



we have already mentioned in treating of methods of preparation we must penetrate pretty far beneath the epithelial surface of the mucosa in these animals before we reach the lymphatics, removing, in most cases, the whole of the superficial regular arborisations of blood-vessels and capillaries that characterise the free surface of the uteri of the bovine and porcine races, and which are absent in the chevaline race, this being due, apparently, in great part, to the great difference in the anatomical structure and arrangement of the uterine glands in these animals, respectively.

In the mare we have immense lymphatics covering almost the whole of the mucosa surface of the internal muscular layer, lying closely packed and parallel to each other. The valvular dilatations are so close and enormous that each lymphatic looks like a string of huge eggs. On the deep surface of the mucosa we have cactus-like ramifications of the lymphatics, as shown in fig. 1, which are similar to those seen in the goat, in fig. 8. These, after innumerable subdivisions and ramifications in the substance of the mucosa, finally appear as terminal vessels, greatly decreased in size, that arrive immediately under the epithelium of the free surface of the mucosa or cavity of the uterus. The great difference in size of the lymphatic vessels as they appear on the respective surfaces of the mucosa is well seen in figs. 1 and 2, drawn under the same power from opposite sides of the same preparation. The terminal superficial twigs of the lymphatics reach the surface either as dilated cul-de-sac endings, like that seen in fig. 3, or as the ramifying sinus shown in fig. 4, descriptive words which are almost identical with those used by Lindgren to describe the lymphatics of the neck of the uterus, and corresponding also tolerably well with his drawings.

All we have described bears not the slightest relation to Leopold's views, to which we may now direct attention under the special heading of

*The Lymphatics of the Uterine Mucosa.*—Leopold's views on this question are specially interesting from the fact that he is the latest investigator who has put the results of his own practical investigations on record, investigations which he conducted under the great advantage of having Lindgren's results, by the same process of preparation, before him to guide or

control his opinions while seeking to verify or disprove the facts put on record by Lindgren. The conclusion he arrived at as to these is definitely stated at page 30, as follows:—"Lindgren has only demonstrated the lymphatics in the cervix uteri, and describes them as loops from which cul-de-sac offshoots with sinuous borders stretch towards the epithelium. These results I cannot confirm, and I shall give my reasons for disagreeing with them further on, when I enter into the other very interesting results of Lindgren in connection with the mucosa."

These reasons subsequently given consist really in the different results Leopold has had from his injections into the mucosa; and as he minutely describes the various steps and appearances of that technique, his descriptions are well worthy of being reproduced, as they not only explain the errors he has fallen into, but fully demonstrate the untrustworthiness of the technique employed for investigating the lymphatics by means of injections. This technique is responsible for nine-tenths of our erroneous conceptions of the lymphatics, two-thirds of what is accepted at the present day, in connection with the lymphatic system, really consisting of the misconceptions due to that method.

Leopold used for his injections a watery solution of Berlin blue, either simple, or mixed with a little gelatine. This he injected, by means of a syringe and fine canula, into the substance of the mucosa, and he describes how, instead of filling distinct lymphatic vessels, as in the case of the muscularis and subserosa, the injection mass became regularly diffused throughout the mucosa, appearing as a circumscribed extravasation. Thus he states, at page 40:—"The mucosa swells up evenly over the point of injection, whether one has entered direct into the mucosa, or between both layers of muscle, and this constantly recurring appearance leads one to suppose that the blue mass has not flowed into separate distinct vessels, but has penetrated completely through the whole tissue of the mucosa. Thus, in the most carefully conducted or repeated injections, a real extravasation takes place, which, however, depends entirely upon the above-mentioned structure of the mucosa. The injection fluid forces its way from the muscularis into the first and nearest spaces formed by a colossal network which might be compared to the framework of a great labyrinthine cavernous

tumour. It forces its way on gradually, from cavity to cavity, till it reaches the surface epithelium, extending itself also laterally; and in this manner are seen in their transverse section injection images corresponding to each other in their chief features, and which only vary in certain points, according to the kind of fluid used."

From the foregoing appearances, Leopold has framed the conclusion in italics which we gave at page 54 of this paper and he explains further on that the mucosa in man and animals consists of a mass of funnel-shaped cavities, having their wide mouths immediately underneath the epithelial clad surface, and gradually tapering to a narrow orifice where they join the lymphatics of the muscularis, the funnel-shaped cavities being longer (or deeper) in man than in other animals. With the drawings of our preparations which accompany our paper, before us, it is almost needless to repeat that all Leopold's conclusions, like the results of the technique upon which they are founded, are nothing but one long series of errors. While in the comparatively firm tissue of the cervix one might not be surprised to learn that Lindgren had some successes together with his mistakes, it is difficult indeed to conceive any result from injection into the delicate, soft, and even plastic, embryonic tissue forming the matrix of the mucosa of the body of the uterus, other than that obtained by Leopold. Even although that mucosa is thoroughly permeated by the normal-valved and endothelium-formed, round-walled lymphatics, as we show it to be, the injection of fluid would at once close all the lymphatics, and form a true extravasation in the tissue, serving very well to lead astray, but not to demonstrate the truth. That there are numerous cavities in the mucosa, we shall afterwards show to be the case; but these cavities have no relation with the ones described by Leopold, and although lined with epithelium, they have no connection whatever with the lymphatic system.

There is this peculiar distinction between Lindgren and Leopold, the only two investigators who profess to have specially investigated the lymphatics of the uterine mucosa, that whereas the latter gives no drawing of his funnel-shaped cavities, his description fully illustrates the grave errors into which he has fallen, while the description of the former probably

represents the exact truth, although his drawings (*vide* his fig. 2) show erroneous conceptions.

While our camera lucida drawings of the lymphatics of the mucosa might be safely left to give a correct idea of their condition in different animals, some little description may be necessary to account for their different relations to corresponding structures in these different animals. In the absence of the fresh human uterus, we have given our special attention to the uterus which (the monkey tribe apart) most resembles it in the structure of its mucosa, namely the uterus of the mare. In the various species the chief differences in the lymphatics of the uterine mucosa are due principally to the great variations in the shape and arrangement of the uterine glands. In the sheep, goat, and pig, these glands may be called compound, as a number of glands open into the one main orifice, and each gland is composed of an immense number of diverticula or cul-de-sac terminations, as shown in figs. 6 and 8. The main orifices referred to open at a considerable distance from each other into the cavity of the uterus, where they appear large enough to be detected sometimes by the naked eye.

In the mucosa of the mare we have a very different condition. The glands are merely single tubes, as in the human subject, and these are so numerous, and open so close to each other into the uterine cavity, that when a lymphatic lies immediately under the epithelium it is in a manner compressed between the glands, and its sides are impacted upon, and distorted in shape by the pressure of the individual glands upon the endothelium-formed walls, whether the lymphatic lies superficially in the plane of the surface, as in fig. 4, or reaches the surface as a villous process or cul-de-sac termination, as shown in fig. 3. At the attached border of the uterus, or, in other words, the side where the broad ligament joins each uterine tube, and where the placenta becomes subsequently attached, the gland tubes or openings are so closely packed that it is almost useless to seek between them for superficial twigs of the lymphatics; but at the opposite or unattached border they are not packed so closely, and consequently the superficial lymphatics are more easily and plentifully found in this locality. The cotyledonous areas in the sheep, cow, and goat, also interfere considerably with the arrangements of the

lymphatics ; and the great difference in the arrangement of the blood-vessels in the animals we have mentioned must cause corresponding modifications in the arrangement of the lymphatics. In those animals, owing to the great distance between the openings of the glands, comparatively large portions of the general surface of the uterine mucosa are left unbroken and undisturbed, so that the blood-vessels which pass as large trunks towards the surface, break up there into a regular meshed network of capillaries, similar to the capillary plexus upon the air-vesicles in the lung. In the mare, however, we have not this extensive and regular capillary plexus, on account of the broken up character of the surface of the mucosa, caused by the innumerable closely packed openings of the uterine glands, and the blood capillaries ramify in a thoroughly irregular manner amongst and between the glands and their openings.

Finally, in the horned ruminants there is a considerable amount of tissue intervening between the superficial capillary plexus we have described and the bulk or secreting portion of the glands ; and although, as shown in fig. 6, from the sheep, the lymphatic loops lie superficial to the mass of the gland tubes yet even then they are some little distance from the surface epithelium. In the mare, however, we have no appreciable layer of tissue intervening between the epithelium and the lymphatics, the consequence being, as already stated, that it is almost impossible to remove the surface epithelium without at the same time injuring the endothelium-formed walls of the lymphatics, which lie immediately underneath the epithelium.

It will, of course, be understood that although in the mare the gland tubes by their pressure upon the wall of the empty lymphatic probably distort the natural appearance of the lymphatic, yet when the latter is full of lymph that distortion may not exist. In the most superficial portion of the lymphatics of the uterine mucosa of most other animals, we do not observe the irregular sinuous borders we see in the mare, as in them the glands are not, so to speak, so crowded upon the lymphatics as to produce any such effect. In the sheep, the irregularity in the shape of the lymphatics seems purely due to functional requirements. The more superficial portion of the loop becoming dilated, and these dilatations being joined together at a

deeper level by narrow portions of the lymphatic vessel, a peculiar appearance is given to the whole plexus when viewed from the cavity side of the mucosa. In the pig, again, we have a similar series of circular loops with the convexities turned towards the epithelial surface of the mucosa, but the arrangement of dilated and constricted portions does not show itself so well as in the horned ruminants. In general, these loops have a distinct thickness of tissue interposed between them and the epithelium, or the superficial capillary plexus. In rare cases, however, the curved loop may approach close enough to the capillary plexus to almost touch it, as seen in fig. 5, where, although the plexus has been removed immediately above the lymphatic loop, yet the points where the various vessels are breaking up into the capillary plexus are still shown in the preparation.

The character of the villous or cul-de-sac projection of the lymphatics, reaching up to the surface epithelium in the mucosa of the mare, is often made clearer by a lucky accident which very often happens when the mucosa of that animal is being prepared for examination. When the mucosa, having been dissected off the muscularis, is being mounted upon the histological rings, the smallest amount of undue distension serves to crack or rupture the sheet of epithelium lining the cavity surface of the mucosa, without tearing the elastic or embryonic tissue which forms the subjacent portion of the mucosa. Where a number of the villous processes referred to penetrate the mucosa in a line with each other, that line forms the weakest part of the surface, and just as riveted iron plates, when forcibly torn apart, give way first at the line of rivet-holes, so does the sheet of uterine mucosa give way in the line of the penetrating lymphatics, showing under the microscope like a deep gash upon the epithelium-formed surface. When the silver is applied after this very common accident, it makes the tissues forming the sides of the fissure very evident, and in this way the blind-ending lymphatic tubes are seen lying upon the side and continuous with the horizontal lymphatics at the bottom of the fissure, a view appearing all the clearer when seen by the binocular microscope.

The changes caused by pregnancy in the lymphatics of the mucosa in the larger animals seem to be very slight and unim-

portant when compared with the changes which take place in the size of the lymphatics lying between the two layers of muscle; in fact, like the other structures in the mucosa, they appear to be pretty much disordered and disarranged owing to the amount of attention given to the part where the placenta is attached. In the non-pregnant uterus the mucosa is comparatively thick, the glands are plump, and the small embryonic or branched cells appear like stars in a clear firmament or matrix of embryonic tissue; the lymphatics also look plump and regular. With the advent of pregnancy important changes occur. The whole of the surface layer of epithelium begins to proliferate, each cell breaking up into several new cells, in order to form the chorion as a sheet of embryonic tissue. The small branched cells become greatly increased in size and elongated into contractile cells that form an irregular felting of cell-fibres of the mucosa; the clear matrix between the cells disappears to a great extent; the blood-vessels increase in size and number; the uterine glands now appear shrivelled and drawn out into very irregular shapes and positions, while the lymphatics appear to be little increased in size or number, although from the great increase in the uterine surface, and from the fact that the lymphatics seem to have the same relative number and size, occupying the same extent of surface, we must conclude that there has been a very great increase in their size, as they became extended over the increasing surface, notwithstanding that in the same sized field of the microscope they appear individually to have increased very little in size. No doubt the lymphatics occupying the whole thickness of the mucosa of the non-pregnant uterus, have, by the thinning of that structure in pregnancy, become compressed and extended into a thin plane which seems to occupy the space between the thin mucosa and the muscularis. Indeed, the mucosa has become so very thin that in the pregnant sheep, from which figs. 13 and 15 were drawn, the silver solution when applied to the free surface penetrated right through to the inner surface of the circular muscular layer, making all the tissues in that thin sheet perfectly distinct as we have just described it.

From what we have said, it would appear that during pregnancy the uterine glands are quiescent, and the embryonic cells

formed into contractile cell-fibres. It seems equally probable that the lymphatics of the mucosa have very little function to perform during pregnancy, for the sheep having a cotyledonous placenta, the lymphatics examined by us lay between the cotyledons, and therefore within the area of the placental functions, although not upon the cotyledons themselves, where we failed to trace superficially the presence of lymphatics. In the pregnant uteri of rodents, being mono-placental animals, we examined at term the site of the placenta for lymphatics, but none were to be seen there, although at the very edge or ridge external to the placental border, a solitary twig, somewhat increased in size, might be observed at rare intervals. Between the muscular layers, however, opposite to the placental attachment, in mice, the intermuscular lymphatics had greatly increased in size, as formerly described. As far, therefore, as these little animals are concerned, parturition can cause little injury to the lymphatic system, but it must constitute a very serious and dangerous condition to those higher animals in whom separation of the placenta may readily injure and lay open those lymphatics lying between the placenta and the muscular wall of the organ.

There is another structure existing in the mucosa which merits attention, and although it is closely allied to many structures that have been described by others, it seems to be, as far as we have been able to discover, hitherto undescribed. It resembles bursal sacs or surfaces interposed between the mucosa and the muscularis of almost all the animals in which we have looked for it. It is not to be detected in transverse sections of the tissue, where it would only appear like the longitudinal section of a uterine gland. To see it properly one must separate the mucosa very carefully from the muscularis, using no cutting of the tissue further than snipping across the blood-vessels, &c., which pass across from the one layer to the other. When this has been done, and either layer or sheet is mounted on the rings, and the surface treated with silver which corresponds to the separation between muscularis and mucosa, it will be observed that large portions of these surfaces are covered with a sheet of epithelial cells. We have found this condition more prominent in the non-pregnant uterus of the pig than anywhere else, and



wherever this covering of epithelium is present it prevents the silver solution from passing through it to act on the tissue underneath. Where there are breakages in the surface or sheet of cells, the solution naturally penetrates for a considerable distance, and renders the matrix very dark in comparison with the white cell-covered portions. These cells are seen to be placed directly upon blood-vessel and lymphatic walls as well as upon the embryonic matrix which forms the bulk of the mucosa; consequently, where it covers these vessels, their endothelium can be no longer seen, although the breaks here and there in the sheet of cells enable one to mark the course and condition of these vascular walls. Similarly, even in a smaller mammal like the guinea pig, these bursa-like sacs appear, more especially during the earlier stages of pregnancy, as oval-shaped sacs or cavities lined by epithelium. The cells themselves resemble in appearance the cells lining the cavity of the uterus, except that they are a little smaller, as if younger. To show the comparative character and size of these cells we have drawn them at *c*, fig. 19, as well as the cells, *b*, lining the cavity of the uterus, and *d*, the cells lining the uterine glands, while *e* and *a* represent respectively the endothelium of the veins and lymphatics of the same locality, all drawn under the same magnifying power by the camera lucida.

These bursal cavities are certainly not funnel-shaped cavities in connection with the lymphatics, or rather forming the mucosa portion of the lymphatic system according to Leopold, for they are flat, collapsed, bursa-like sacs, and not funnel-shaped, extending across the mucosa. They have no connection whatever with the lymphatics, and the characters of the cells lining these cavities are the very opposite of the cells belonging to the lymphatic endothelium. At the same time it is very possible that these were the cavities injected by Leopold; in fact, he could scarcely avoid them, and if injected into they would certainly give the distinctly circumscribed borders that he has referred to. We have already shown abundantly that Leopold's conclusion that the lymphatics of the mucosa were not really the round vessels that one meets with everywhere else, is entirely erroneous, for the lymphatics in this structure are exactly like the lymphatics elsewhere in the body. In short,

we must come to the conclusion that Leopold had really injected these bursal cavities or sacs, and endowed them with the attributes of the lymphatics, when he failed to find the real lymphatics of the locality. At the same time, we have still to find a function for such sacs. It is possible that they really have the function of bursæ, to allow of the mucosa gliding upon the muscularis for some purpose or other, although the shape of the cells does not correspond with that of cells usually lining bursal sacs. We are, however, of opinion that there is still another reason for their presence, and that it is from these sacs that the new epithelium is derived with which to line the cavity of the involuted uterus after parturition. This opinion is merely advanced as an hypothesis, but it seems to us to be a very probable one under the circumstances.

*The Lymphatics of the Uterine Muscularis* forms the second of the two divisions under which we class the uterine lymphatics, and, as we stated at the commencement, we include under that head those lymphatics which have hitherto been described as the subserous lymphatics. Under that head is really comprised also all that was definitely known of the uterine lymphatics prior to the researches of Lindgren and Leopold. Lindgren, however, does not enter into the consideration or give any description of any others than the lymphatics of the mucosa of the neck, so that Leopold ought to be considered as the first, and indeed the only, investigator who has offered a systematic description of the lymphatics of the muscularis. As mentioned in our historical *resumé*, many writers speak of the superficial lymphatics on the peritoneal surface of the organ as coming out of the muscular substance, but their character or relationship there, like the question of their existence on the mucosa beyond, could only be hypothetically discussed in the complete absence of ascertained facts.

Following Leopold, we have, five years afterwards, another investigator, Dr Vladislav Mierzejewski, already referred to, who, under the title of "Investigations into the Subserous Layer of Lymphatics of the Uterus," really gives an excellent description of the lymphatics of the superficial or longitudinal muscular layer of the normal-sized organ in the sheep. Our first task

will therefore be to show that such a division as the subserous lymphatics is erroneous, and that the division ought to be absorbed into that of the muscularis.

Commencing with Mierzejewski as the latest investigator, we have first to observe that his drawings and descriptions are singularly opposed to his conclusions, even when the two latter are given in the same paragraph, which we quote *verbatim*, in order to allow our readers to judge for themselves. If, in addition, they have access to the article in question, one glance at the drawing he gives of the lymphatics, as seen in transverse section, ought to convince any one that we have there to deal with the lymphatics of the superficial muscular layer, and not with those of the subserous tissue only, for they form a special and regular plexus of enlacement of the muscular bundles constituting that layer. With respect to this, he states, p. 220: "In successful examples of subserous injections, it is seen that the subserous lymphatics having been filled, the injection mass passes into the lymphatics which are distributed to the muscular layer, and it is from these latter, and not from the subserous lymphatics, that the great lymphatic trunks arise which pass towards the broad ligaments. It is generally admitted that the uterine lymphatics have their origin in the mucous layer, and go from there towards the superficies of the organ, from which they pass downwards, as great trunks, into the broad ligaments." After this remark, with the correct description which precedes it, comes now his conclusion in the opposite direction: "It cannot be considered that the lymphatics of the subserous layer are in continuation (*forment suite*) with the lymphatics of the muscular layer. They appear rather to constitute a separate plexus which joins the lymphatics of the muscular layer at the level of that layer, and the lymph thus mixed descends towards the broad ligaments." It is difficult to understand this self-contradictory conclusion, which is entirely at variance with the previous description and the drawings given, and, we may add, equally at variance with the facts of the case, as we have ascertained them to exist in the same animals by the silver process, which coincide more or less with the drawings already referred to. In short, as drawn by Mierzejewski, the so-called subserous lymphatics are evidently only

the superficial portion of the lymphatic plexus which encircles and embraces the muscular bundles of the longitudinal muscular layer. It is much to be regretted that he only offers one complete mesh of the subserous lymphatics in his fig. 1. Such a drawing may show the size of the lymphatics, and proves that he actually saw them, but it gives a false idea of the general distribution, a point indeed always left vague by the injection method, which may either multiply the supposed lymphatics by extravasation, or fail to show any where many may exist. It is for this reason that we show the lymphatics on a comparatively large portion of the surface, but even then it is to be borne in mind that the portion has been specially selected as showing the largest number of lymphatics in the specimen, and that in the same specimen there are areas, equally large, where not a single lymphatic appears in the subserosa.

When, next, we turn to the long chapter which Leopold devotes to the lymphatics of the subserosa, we find that he too gives descriptions which lead us to suppose that he clearly understood the relationship between those of the subserosa and muscularis, but takes it for granted that they are to be described separately, although no reason for doing so is apparent. With respect to this he distinctly states: "*From the periphery of the uterus the lymph passes through the muscular lymphatics in order to reach the trunks in the parametrium.*" Nowhere does Leopold seek to prove that the lymphatics of the subserosa are a plexus apart. While he recommends the injection method for investigating the lymphatics, he dwells strongly upon the tendency to cause extravasation that he has experienced—a caution not out of place, seeing that some of his best drawings are from extravasations, and unconnected with the lymphatics. All the drawings in his first plate are devoted to the subserous lymphatics; but as these figures are only of the natural, or even half the natural dimensions, it is difficult to trace the exactitude of the partial injection represented. We are of opinion, however, that these injections are incorrect as compared with the results we have obtained by the silver process, and if his fig. 7, the only drawing of the subserous lymphatics in transverse section which Leopold gives, is to be considered as a

portion of either of the uteri shown in the first plate, then there can be no doubt that these figures are incorrect, and that the greater number of the lymphatics of the subserosa and superficial muscular layer represented there are only extravasations. From numerous silver preparations made by us on the same animals, both in the normal and pregnant uterus, we feel inclined to hold that no such lymphatics exist as are shown in the upper portion of his fig. 7. On the other hand, his drawing of the subserous lymphatics of the uterus of the rabbit, as shown by injection of silver solution, seems to be quite correct; but the numerous short trunks, disappearing as they pass downwards through the muscular layer, show that we have in that plexus only the superficial portion of a plexus which exists equally dense between and below the bundles of muscles forming the superficial layer. In short, it is almost possible to prove from the researches of those who profess to describe the lymphatics of the subserosa as a system apart, that those lymphatics are not anatomically those of the subserosa, but of the superficial muscular layer.<sup>1</sup>

When we turn to Leopold's special references to the lymphatics of the muscularis, we find them distorted by the mistakes made by the injection method. He carefully distinguishes between the lymphatics of the regularly arranged double layer of muscles in the lower animals and the irregular musculature in man, and his fig. 8 is supposed to represent the lymphatics

<sup>1</sup> After the plates and text of the present article had been arranged, and were on the point of being sent off for publication, one of us while in Paris, and through the kindness of Professor Ranvier, of the Collège de France, in placing the use of the laboratory of histology at our disposition while there, had an opportunity of applying our methods of preparation to the human uterus a few hours after death. In it, notwithstanding the unfavourable condition of the tissues caused by the intense heat of the season, we were able to investigate fully the condition and relations of the so-called subserous lymphatics, and to recognise the presence of those of the mucosa. These subserous lymphatics are to be found most plentiful where the peritoneal tissue is loosely attached to the neck of the uterus, but as we approach the upper part of the body of the organ where the peritoneal tissue becomes, so to speak, firmly incorporated with the uterine tissue, the lymphatics are not found at all superficially. Further down on the neck, where the loose peritoneal tissue may be easily dissected off the organ and suitably prepared, the lymphatics form an intricate network intertwining irregularly with the plexiform arrangement of long narrow bundles of smooth muscles, which pass in every direction across the surface of the organ, and give so special an appearance to the subserous tissue of the human uterus and broad ligaments.

of the musculature of a young woman. With regard to this drawing we desire to record our conviction that it does not show a single lymphatic, and that the beautiful plexus of supposed lymphatics shown there is the result of extravasation, and has no connection with the lymphatic system. For our part we show two drawings of the lymphatics of the musculature of the uterus of a woman, as seen in thin section, one section being through the anterior part of the wall at right angles to the uterine cavity, the other being perpendicular through the musculature of the neck, and parallel to the cavity. These preparations were obtained by a natural process that has often been utilised, and deserves more recognition and employment. It was obtained from a patient who died of cancer, which had appeared secondarily in the womb, and had there first shown itself in the glands of the mucosa, and afterwards infected the glands.

In an article published in the *Archives de Physiologie* for 1881, we have shown the course followed after infection of the lymphatics of the skin by cancer, and we have found that the same description applies equally well to the lymphatics of the uterus. According to that process we find that when the lymphatic vessels become infected in the immediate neighbourhood of the cancer, the effect becomes at once manifest in the nearest lymphatic gland, which bars further passage to the lymph-cells passing down the lymph stream, and these become aggregated within the lymphatics, filling them with a solid and tenacious plug of cells, which extends not merely between the gland and

These are the bundles which form the apparent fibres and ganglions which have been dissected out upon the gravid uterus of woman by Tiedemann, Robert Lee, and Snow Beck, and described by them to be the hypertrophied nerves of the uterus. These researches gave rise to much acrimonious dispute at the time between the latter two gentlemen, and obtained for the last named the gold medal and fellowship of the Royal Society. As the error committed by them seems never to have been explained, we venture to give this explanation, with the remark, however, that apart from the great error of describing such muscular structures as nerves, the beautiful dissections of Dr Beck, which he has often personally shown and explained to us, will always remain valuable as an artistic dissection of the subserous muscular arrangement of the human uterus when hypertrophied by pregnancy. The loops formed by the lymphatics which intertwine with these muscular bundles and with innumerable dilated blood-vessels, lie with their long axis transversely upon the organ, and only in the particular locality referred to. It is now too late to attempt to introduce drawings of these lymphatics into the present article, but these and others may subsequently be formed into a supplementary research for future publication.

the original seat of the cancer, but also beyond the latter structure, until the whole lymphatic system of the part is in a manner naturally injected with cells which rapidly become cancerous. In this condition they imbibe deeper staining than the surrounding tissues, so that when sections are made after preliminary injection of blood-vessels we have a complete and perfectly uninjured plan of the lymphatic system of the part. From such a plan figs. 17 and 18 have been drawn. Of course it is necessary to select a case where the uterus is unchanged in shape or dimensions, and where the disease has neither stopped short of, nor gone beyond, the mere filling of the lymphatics, and then nothing can be more satisfactory or correct than such preparations. In the drawing in question the lymphatics have been left blank, as being better than drawing the mere cast of cells within them.<sup>1</sup>

The lesson taught by such preparations is that, in the musculature of the human uterus, the lymphatics, like the muscular bundles amid which they lie, are extremely irregular in course and distribution. They bear no special relation either to the blood-vessels or to the muscular bundles, and their size and shape vary very little in the whole musculature intervening between serosa and mucosa, except where, near the attachment of the broad ligaments, they join to form the great efferent trunks passing from the organ.

In fig. 8 of Leopold it is perfectly evident (even by comparison with his own fig. 7 of the lymphatics of the musculature of the sheep) that he has formed an interstitial injection of the part, so as to separate the different parallel muscle bundles

<sup>1</sup> This complete injection or plugging of the lymphatics by even normal lymph-cells is in our opinion a by no means uncommon, although we believe hitherto unrecognised, general feature. Indeed, whenever either from tubercle, scrofula, or any infective process the lymphatic glands become swollen, that condition seems generally to be accompanied by more or less filling of the lymphatics leading to the swollen glands; in other words, where the lymphatic passage becomes barred at the glands, the wandering cells coming down the lymph-stream gradually collect first in the gland, and afterwards upwards from it, until the whole of the lymphatics leading to the gland or glands become plugged with cells. A short time ago Professor Sappey showed us such specimens of cell-filled lymphatics in the human intestine, which he considered to be a normal condition, and connected with a theory he will shortly publish. To us they simply betokened a swollen condition of the mesenteric glands, in fact "*tabes mesenterica*."

from each other, and to fill up the interstices with the injection mass of gelatine. In the preparation thus formed, where the muscle bundles are, as it were, embedded in a matrix of coloured gelatine, a section across the axis of the bundles shows them, as it were, separated from each other by lymphatic spaces, a condition naturally non-existent, incorrect, and misleading. In his fig. 7, the only other drawing of the musculature given by Leopold, it is equally plain that although the lower half of the drawing corresponding to the lymphatics of the circular layer is probably quite correct, the upper half, corresponding to the lymphatics of the longitudinal coat, is certainly not so, but is an excellent example of extravasation in the interstices of the muscular bundles, showing apparently vessels which are certainly not lymphatics. Indeed, we fail to recognise that figure as representing the lymphatic arrangements in the uterus of the sheep, as far as similarity with either our own preparations or the drawings of Mierzejewski goes. The appearances due to extravasation appear to have also caused Leopold to use descriptive terms which the general similarity throughout the lymphatics of the musculature scarcely calls for. Independently of the terms signifying lymph-vessels, collecting tubes, efferent channels, main trunks, &c., he introduces such terms as *Lymphspalten* (lymph-clefts), *Lymphröhren* (lymph-tubes), which in the way he applies them are evidently only forms of extravasation.

While in the smaller mammals the principal and largest lymphatics lie between the two muscular layers, and next to these in development are the branches passing off from them to the peritoneal surface, the branches passing towards the mucosa surface being almost *nil*, these conditions become very much altered as we pass to the larger mammals. In these, owing to the great development of lymphatics in the mucosa, the lymphatics on that surface of the musculature arrive at the greatest degree of development, while the branches passing towards the peritoneal surface remain either unchanged in their comparative amount or become even less. It is, moreover, in the main lymphatic trunks of the intermuscular layer, and those which in the larger mammals lie on the mucosa surface of the circular muscles that we see an obvious provision for the changes



which take place in pregnancy. The series of shapeless and compressed valvular dilatations, seen in fig. 14, from the pig, is evidently so designed that, when distended by pregnancy, regular intervals of uniform straight tubes or vessels shall intervene between the different valvular dilatations, as is indeed showing itself in fig. 15 from the same locality in the pregnant uterus of the sheep. In both these drawings the portion of lymphatic represented has nothing exceptional about it, but resembles in form and size numerous other lymphatic streams lying parallel to each other in their respective preparations. Of course there is an immense increase in length and calibre at the same time by the addition of endothelial cells interposed between the original endothelium forming their walls in the normal uterus; and in order to make this increase more evident we have drawn fig. 14 from the normal uterus of the pig, under the same magnifying power as fig. 15 from the pregnant uterus of the sheep (the foetus being half-grown), and shown in fig. 16, so that figs. 15 and 16, from similarly sized animals, may be compared, to show the change in size which has taken place at only half the distension of full pregnancy.

In the case of the mare, we have already compared the lymphatics lying upon the mucosa surface of the musculature to strings of eggs lying parallel to each other, and so closely packed together as to form a kind of mosaic of great oval valvular sinuses covering completely the surface of the muscle. These valvular sinuses were relatively similar both in size and shape to each other, and thus presented a different appearance from the distorted and irregular valvular sinuses, seen in the case of the pig in fig. 14. The long axes of these oval valvular sinuses were parallel to the fibres of the circular muscles upon which they lay, but their peculiar oval shape, as well as the short and dilated condition of the valvular sinuses of the cactus-like arborisations of the lymphatics on the deep surface of the mucosa, as seen in figs. 1 and 8, are evidently well fitted to become elongated into shapely lymphatics without any addition to the number of their valves when the distension of pregnancy supervenes.

Although figs. 14 and 15 are drawn from opposite surfaces of the circular layer of muscle, they represent the same kind of

lymphatics, which in these animals lie rather between and parallel to the bundles of muscle in the circular, than between the circular and longitudinal layers. At the top of fig. 14 is also seen the cut lymphatic, where the vessel became continuous with the long parallel channels which characterise the deep lymphatics of the mucosa of the pig. In the case of the mare, where a special layer of lymphatics, as formerly described, covers the whole inner or mucosa surface of the musculature, the branches pass upon the deep surface of the mucosa, at various points in the circumference of the uterine tube, in order to form the plexus seen in fig. 1; but there is nothing abnormal, in any case, in the manner in which the lymphatics of the musculature join those of the mucosa. We should not even have referred to this point, had it not been that Leopold, misled by the non-success of his injections of the lymphatics of the mucosa, brings up misconceptions about funnel-shaped cavities, whose small ends or orifices join at this point with the lymphatics of the muscularis. Thus he states, at page 48: "At the boundary of the muscularis the lymph-spaces (of the mucosa) enter for a short distance (in human uteri farther than in those of animals) into the funnel-shaped cavity between two muscle bundles, and there gradually become narrow vessels to join the intermuscular lymph-vessels and sinuses." As, except by a hypothetical opinion of the cause of the failure of the injection method, there is no foundation for stating that funnel-shaped lymph-cavities exist in the mucosa with their narrow portions joining the lymph-vessels and sinuses in the muscularis, it is curious to be told that such hypothetical narrow ends pass deeper, in the case of the human uterus, into the musculature before joining the lymphatics of the musculature.

Leopold devotes fourteen closely printed pages to his description of the lymphatics of the musculature, but the whole being hypothetical, and founded in great part on injection extravasations into the tissue, we may be excused quoting it to show the wonderful exactness introduced by Leopold into his description of non-existent lymph-spaces. In his conclusion, he, however, states (p. 48) of the lymph-vessels and lymph-spaces in the musculature: "The walls of both are formed of the intermuscular connective tissue. The former are lined by a fine endo-

thelial lamella which possesses openings and holes here and there. The latter are lined with thick cell-plates."

In respect to these opinions, we may state that the walls of the lymphatics throughout the whole organ, like the lymphatic walls throughout all the organs in the body (this does not refer to the great efferent systemic lymphatics), are formed or lined by the same crenated-edged endothelium, and that, except in the increased number of these cells, pregnancy itself makes no difference or addition to these lymphatic walls, while as to the openings, holes, or stomata, to which Leopold gives such importance in his research, and over which Dr Lucas Championnière rejoices, as a fulfilment of his prophecy, there are none whatever, except where they have been made in the preparation by the preparer himself, or are simulated by small deposits of albuminate of silver. In the hundreds of specimens we have made and examined of the lymphatics of this organ (or of any other organ) in different mammals, we have never seen anything corresponding to a stoma or natural opening. We cannot even understand for a moment what function it would subserve which is not already exercised by the lymphatics; while, as far as any stomata exist in mammals connecting the peritoneal cavity with the lymphatics on the peritoneal surface of the musculature, the idea is as hypothetical and mythical here as when applied to other serous surfaces throughout the body.

We almost feel ashamed to have so often to recapitulate our disbelief, or rather our denial of the existence of stomata in connection with the lymphatics; but it is almost impossible to take up any German research on the lymphatic system without meeting some kind of assertion as to their presence or existence. Like Sappey (*loc. cit.* p. 794), we "long to see those stomata which will, after having shone with an ephemeral brightness, go modestly to join their ancestors, the absorbing mouths, which William Hunter thought he saw upon the villi, which Haase thought he had discovered on the surface of the skin, and which Mascagni believed to exist on the convex surface of the liver, those open mouths, always disproved, and yet always reappearing in some other form in the history of our errors, and which still remain as examples of the unfortunate tendency of many otherwise eminent minds to be led astray by

illusions and vain theories when under the influence of some preconceived idea."

Before summarising our conclusions we wish specially to record our indebtedness to the kindness of Mr Bartlett, the well-known and able superintendent of the Zoological Gardens of London, who has taken a great deal of trouble to provide us with most of the material upon which the present and previous researches published in this Journal have been made.

#### CONCLUSIONS.

1. In the uterus, the lymphatics are found principally as layers connected with each other, and corresponding to the distinct layers of tissue in the uteri of the lower animals.

2. In monkeys and in mankind, the complex arrangement of the muscular bundles leads to a corresponding irregularity in the arrangement of the lymphatics of the musculature, but even there the principle of layers, corresponding to the bundles of muscle, can be clearly recognised.

3. The complexity and amount of the lymphatics increases as a rule very distinctly as the size of the class of animals increases.

4. In rats, mice, and similar small mammals, there is only one main plexus lying between the circular and longitudinal muscles. From this plexus small twigs pass around the outer or longitudinal muscular layer, but return again to the plexus. These have been erroneously spoken of as the lymphatics of the subserosa. Few or no lymphatics pierce through, or appear on, the mucosa surface of the circular layer of muscles. Their mucosa possesses few or no lymphatics.

5. In medium-sized animals, like the sheep or goat, the lymphatic twigs passing to the subserosa retain their comparative amount and size, but the circular muscle is now pierced by large lymphatics which ramify in the mucosa. The larger portion of the lymphatics of the mucosa ramify like a cactus-plant on its deep surface, and send loops towards the free surface, from which, however, they are generally separated by a certain thickness of tissue, and a very regular and complete blood-capillary plexus.

6. In large mammals, like the mare, no lymphatics appear on

the peritoneal side of the musculature, but a great development of lymphatics takes place on the mucosa surface and within the mucosa itself. The lymphatic plexus on the deep surface of the mucosa is formed by immense valved vessels, which send off branches both as loops and villous processes, that pass right up to the epithelium of the inner surface of the uterus. Under that epithelium the villous processes end, but the loops often ramify as sinuous channels between the openings of the uterine glands.

7. The changes caused by pregnancy are best marked in the intermuscular layer of lymphatics, which increase greatly in size, but not appreciably in number. The changes are marked to a much less extent, both in the lymphatics of the mucosa and of the longitudinal muscular coat.

8. The uterine glands have no connection with the lymphatics, either in the pregnant or non-pregnant uterus, and their function seems entirely suspended during pregnancy.

In the subserosa of all animals, there are no lymphatics having their commencement there, as a collecting plexus. The small twigs found there come from the deeper lymphatics of the intermuscular plexus, and after a very short course return thither, so that there is no ground for speaking of these twigs as belonging to a separate category of subserous lymphatics.

## DESCRIPTION OF PLATES I. AND II.

### LYMPHATICS OF THE UTERINE MUCOSA.

*(Drawings made by the aid of the Camera Lucida.)*

Fig. 1. Sheet or plexus of huge valved efferent lymphatics lying upon the deep surface of the uterine mucosa in the mare,  $\frac{1}{1}$ .

Fig. 2. Terminal sinuous twigs of superficial and valveless collecting lymphatics, appearing immediately underneath the lining epithelium of the uterine mucosa in the mare. These twigs are the ultimate radicles of the plexus seen in fig. 1, both figures having been drawn under the same magnifying power, and representing the opposite surface of the same mucosa,  $\frac{1}{2}$ .

Fig. 3. View under high magnifying power of the extreme point of one of the villus or cul-de-sac terminations of the lymphatics on

the free surface of the uterine mucosa, immediately underneath its covering epithelium, showing specially its relations to the openings of the uterine glands in the mare,  $\frac{1}{180}$ .

Fig. 4. Magnified view of a portion of one of the terminal twigs of the sinuous variety seen in fig. 2, in order to show its relations with the glands, blood-vessels, and branched cells of the locality,  $\frac{1}{80}$ .

Fig. 5. Summit of the arch-like bend of one of the superficial lymphatics of the uterine mucosa in the pig, where the lymphatic approaches nearest to the surface but still lies underneath the regular superficial blood capillary network characteristic of this and of the bovine animals, but which does not exist in the mare. In this animal also the gland openings are so far apart that none of them appear in the same field with the lymphatic, and only the larger branches of the blood capillary plexus have not been removed by the scalpel,  $\frac{1}{80}$ .

Fig. 6. Superficial portion of the lymphatics of the uterine mucosa in the sheep, forming loops or arches dilated at their centres. These loops do not come so near to the lining epithelium as on the mare, but lie between the branching uterine glands and the surface epithelium,  $\frac{1}{80}$ .

Fig. 7. View under a low power of the terminal loops in the sheep, of which fig. 6 is a portion highly magnified,  $\frac{1}{12}$ .

Fig. 8. Highly magnified view of a portion of the plexus, of efferent lymphatics lying on the deep surface of the uterine mucosa in the goat. This plexus corresponds to that seen in fig. 1 from the mare, and it is intended as a companion to fig. 6, in continuation of the superficial lymphatics of the mucosa in the sheep, the character of the uterine structure being similar in the two animals,  $\frac{1}{80}$ .

The following letters apply equally to all the above figures:—*l*, lymphatics; *g*, uterine glands; *c*, branched cells; *v*, blood-vessels.

#### LYMPHATICS OF THE UTERINE MUSCULARIS.

Fig. 9. View under very low power of the lymphatic twigs appearing on the outer or subperitoneal surface of the longitudinal muscular layer of the non-pregnant uterus of the mouse, showing that these belong to the muscle and not to the subserosa,  $\frac{1}{12}$ .

Fig. 10. Highly magnified view of one of the twigs seen in fig. 9, in order to show comparatively the changes effected in the size of the endothelium and calibre by pregnancy, by comparison with fig. 11,  $\frac{1}{80}$ .

Fig. 11. From the same locality and under the same magnifying power as fig. 10. From the pregnant uterus at term in the mouse,  $\frac{1}{80}$ .

Fig. 12. View under low power of the lymphatic twigs on the subperitoneal surface of the longitudinal muscular layer of the non-pregnant uterus of the goat, for comparison with fig. 13,  $\frac{1}{10}$ .

Fig. 13. View under the same power as fig. 12 of the same description of lymphatics dilated by pregnancy in the sheep, the foetus being a little more than half grown,  $\frac{1}{10}$ .

Fig. 14. View under high power of the lymphatics belonging to, and lying parallel with, the circular layer of muscle of the non-pregnant uterus of the pig. This figure is drawn from the mucosa side of the

muscular layer, although it really lies in the same plane with that layer.

Fig. 15. View under very low power of the lymphatics or lymphatic of the circular muscular layer in the pregnant uterus of the sheep, from which fig. 13 was drawn, in order to show the dilatation caused by pregnancy in two similarly sized animals like the sheep and the pig. Fig. 14 has been reproduced in fig. 16, under the same magnifying power as fig. 15, for comparison with the latter,  $\frac{1}{10}$ .

Fig. 17. Transverse section through the musculature of the body of the human uterus, showing the lymphatics injected by cancer cells in section, and their relationship with the dilated blood-vessels,  $\frac{1}{12}$ .

Fig. 18. Antero-posterior section through the front wall of the same uterus as in fig. 17, in the mesial line of the neck, showing that even in the irregularly arranged musculature of the human uterus the lymphatics still appear to be in layers (diagonally across the drawing),  $\frac{1}{12}$ .

In the above the following letters apply throughout:—*e*, peritoneal endothelium; *l*, lymphatic; *m*, muscle; *v*, blood-vessels.

Fig. 19. Comparative view of the size of various endothelial and epithelial cells, having special reference to *c*, the cells lining the great bursal cavities lying between the uterine mucosa and muscularis; *a*, lymphatic endothelium; *b*, lining epithelium of the uterine cavity; *d*, lining epithelium of the uterine glands; *e*, venous endothelium,  $\frac{1}{100}$ .

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A CASE OF TRANSPOSITION OF THE AORTA AND  
PULMONARY ARTERY IN A CHILD OF SEVEN  
MONTHS. By HENRY ASHBY, M.D. Lond., *Lecturer on  
Diseases of Children at the Owens College.*

AMONG the varied forms of congenital deformities of the heart, no one is more curious than that which is distinguished by the aorta arising from the right and the pulmonary artery from the left ventricle. This reversal of the natural arrangement is by no means common, still numerous instances are to be found recorded since the beginning of the century. Dr Peacock in his work on *Malformations of the Heart* gives references to some twenty cases. Lewis Smith has collected fourteen, though very probably he has included some of the above. A typical case, with plates and description, will be found in Förster's *Missbildungen des Menschen*. Others will also be found scattered through the pages of the journals. The one which survived the longest that I have been able to find is one referred to by Dr Peacock, which lived two years nine months, but for the most part such monstrosities terminate their existence in a few weeks to a few months. The following case, which survived seven months, is perhaps worthy of being recorded :—

Mary O., aged two months, was admitted to the Dispensary for Sick Children, Manchester, in December 1880. Her mother stated that at birth she was very white, but in two days became blue, and has remained very blue ever since. She was small for her age, feeble, wasted, sluggish in her movements; her face had a pale blue tinge, while her tongue, mucous membrane of the mouth, and extremities, were of a dusky blue colour.

The heart sounds were normal but weak, and there was no bruit. The breath sounds were normal, but there was some want of resonance under the left clavicle. The child came regularly to the dispensary from week to week; it remained small, feeble and cyanotic; its growth was apparently slow; it suffered at length from stomatitis, and died in the following May at the age of seven and a half months.



*Post-mortem.*—On opening the chest it was apparent that the shape of the heart was peculiar; its right margin was more rounded in outline than usual, and the right side occupied an abnormally large portion of the anterior surface. It was seen that the aorta was arising from the right ventricle, and the pulmonary artery, much smaller than the aorta, was situated behind the latter and arose from the left.

Both vessels after leaving the heart pursued a normal course. The ductus arteriosus was impervious. The lungs were healthy. On opening the cavities of the heart the following notes were made:—

*Right Auricle.*—Capacious cavity, occupying nearly the whole of the base of the heart, both anteriorly and posteriorly, well developed auricular appendix. Superior and inferior venæ cavae normal. Foramen ovale consisting of a vertical slit at the anterior aspect of the fossa. Eustachian valve hardly a trace left.

*Left Auricle.*—Very small cavity, receiving the pulmonary veins and constituting simply a dilated channel conveying the blood from the pulmonary veins to the open foramen ovale and left auriculo-ventricular opening. The appendix is rudimentary.

*Right Ventricle.*—Much more capacious than left, with walls more than twice as thick, well-marked columnæ carneæ; auriculo-ventricular opening much larger than left, has three cusps, one forming a hernial protusion through an opening at base of septum into left ventricle. Aortic opening at upper part of the cavity.

*Left Ventricle.*—Much smaller than right, walls thinner, apparently cannot contain more than one-fourth the amount of the right; auriculo-ventricular opening about one-half the diameter of the right, provided with two small cusps. Opening of pulmonary artery about one-half diameter of the aortic. Immediately below this orifice is a foramen or spot where the ventricular septum is wanting. A similar foramen, though smaller, is present in the septum a short distance below.

With the above arrangement, it is obvious that during life there existed two distinct circulations. The one circulation of blood from the right auricle into right ventricle, thence *via* aorta, arterial, capillary, and venous systems back to the right auricle.

The other from left auricle to left ventricle, and thence *via* pulmonary artery, capillaries of the lungs, and pulmonary veins, to the left auricle again. It is perfectly certain that if no inter-communication existed through the septa between the two sides of the heart, that venous blood would continually circulate in the one and arterial blood in the other. But in this case an open foramen ovale permitted blood from the pulmonary veins to mingle with the venous blood of the right auricle, and so leaven to a certain extent the venous blood distributed to the system, while the imperfect ventricular septum allowed some of the venous blood of the right side to enter the pulmonary artery and so reach the lungs.

From the cyanotic appearance of the infant and a consideration of the above arrangements, it is quite certain that a large proportion of venous blood was distributed to the general system.

During foetal life, the flow of blood through the foramen ovale must have been in the normal direction, *i.e.*, from right to left; while after birth the blood must have flowed in the opposite direction, *i.e.*, from left to right. Before birth the blood of the inferior vena cava, must have been distributed by the pulmonary artery through the ductus arteriosus and descending aorta to the lower extremities, instead of through the aorta and carotids to the head, the lower extremities and lungs thus getting the purest blood in the body. The narrowness of the pulmonary artery, and consequent distended condition of the left side of the heart, would tend to prevent the complete closure of the intra-ventricular septum, and on the other hand after birth allow of the obliteration of the ductus arteriosus.

For an explanation of these curious cases we must look to embryology. Prior to the seventh week of intra-uterine life the heart consists of three cavities, a ventricle, an auricle, and the aortic bulb. About this time a septum is gradually formed in each leading to the formation of two ventricles, two auricles, and two canals, the aorta and pulmonary artery. At the same time that the ventricular septum is growing from below upwards towards the base of the heart, the partition which converts the aortic bulb into aorta and pulmonary artery is being formed from above downwards, and the heart undergoing more or less

of a spiral movement, the anterior division of the aortic bulb or pulmonary artery is applied to the anterior or right division of the ventricles, and the posterior division or aorta is applied to the left. It is not difficult to imagine that an abnormal twist of the ventricles or aortic bulb, or an unusual growth of either septa should be the means of causing a transposition of the large vessels.

The survival of the foetus would depend upon the extent to which the foramen ovale remained open, and the ventricular septum incomplete, to allow of an intermingling of the arterial with the venous blood.

*Size of the various Openings.*

Foramen ovale, . . . . .	10 mill. in diam.
Right auriculo-ventricular, . . . . .	15 " "
Left do. do., . . . . .	7 " "
Aorta, . . . . .	12 " "
Pulmonary artery, . . . . .	5.5 " "
Upper inter-ventricular foramen, . . . . .	5 " "
Lower do. do., . . . . .	4 " "

## ON SOME POINTS IN THE HISTOLOGY OF THE NEWT.

By WILLIAM STIRLING, M.D., SC.D., *Professor of the Institutes of Medicine in the University of Aberdeen.*

DURING the course of last summer session I had occasion to investigate the various tissues of the common newt, and I am able to confirm fully the results of the various observers who praise the newt so highly for histological purposes. I would call attention to two preparations which I find to be particularly instructive to students, as illustrating many even of the most recent discoveries in the structure of cells and nuclei.

*The nuclei of striped muscle of the Newt.*—I find the striped muscular fibres of the newt are particularly good for demonstrating the nuclei with their well-marked intranuclear plexus of fibrils, which can be shown with the utmost clearness and distinctness.

*Method.*—Place very small portions of any striped muscle—from the back or a limb—in a five per cent. solution of ammonium chromate for twenty-four hours. After this time wash away the surplus chromate, and tease a small piece in picrocarmine, and mount it in glycerine containing a trace of formic acid. Seal up the preparation, and after two days examine it, when the very long fusiform nuclei will be found stained of a deep red. Within each nucleus one can see with the utmost clearness the intranuclear plexus of fibrils. On comparing the plexuses within different nuclei, it is easy to make out marked differences in the arrangement of the fibrils. As in the frog's muscle, these nuclei are scattered throughout the whole thickness of the sarcous substance; but this arrangement is better seen in transverse sections of the fibres. I find, however, the following method yields excellent results:—After teasing a small piece of muscle in picrocarmine, wash away the surplus picrocarmine, and stain the morsels of tissue in a watery solution of iodine green in the usual way, and mount them in Canada balsam or dammar. The advantage of this double staining is this,—that the sarcous substance is stained red, whilst the nuclei stand out of a bright green. The intranuclear plexus is again distinct, and it is easy to convince oneself that the fibrils

are embedded in an interfibrillar substance, which is deeply stained with the iodine green.

*Tail of the Newt.*—Another preparation which I find most useful for class purposes, is a transverse section of the tail of a newt, which, containing as it does so great a variety of tissues, forms a most interesting and instructive object of study for the student. Klein has already described (*Quarterly Microscopical Journal*) the characters of the glands and epithelium of the skin of the newt, and given minute details regarding them and their structure, and also indicated a modification of the picric acid method for their demonstration. In preparing the tail of the newt for microscopic examination, one may employ either the method indicated by Klein, or use a five per cent. solution ( $\frac{1}{2}$  per cent.) of chromic acid, to which one-third its volume of methylated spirit has been added. A few days suffice to harden the tissues if the latter fluid be used. Transverse sections are made, and may be stained with picrocarmine and mounted in glycerine, or doubly stained with picrocarmine and iodine green, as indicated for newts' muscle. The advantage of this preparation is, that it shows so many tissues. The epithelium, with its large nuclei with an intranuclear plexus, in several layers is seen on the surface; and at the base of this, or scattered amongst the deeper epithelial cells, it is easy to find branched pigment cells. The simple saccular glands, with a mouth opening on the surface between the epithelial cells, are specially instructive to the student, because they offer an example of a secretory gland of a most simple type; and, as Klein showed, the cells lining these glands are remarkably large, and so are their nuclei, so that it is easy to demonstrate the existence of the fibrillar plexuses within them. Numerous sections of muscular fibres are met with, and the nuclei are very large and scattered throughout the sarcous substance; some are under the sarcolemma, and others are embedded in the sarcous substance. They stand out prominently in the doubly-stained specimen. The connective tissue, or *endomysium*, is particularly instructive, for it assumes a well-marked lamellar form with large connective tissue plates between the lamellæ. Many fat cells and a portion of bone—the section of a vertebra—will be found; and if the section is taken from the tail near where it was detached from the trunk, one obtains a section of the posterior extremity of the spinal cord.

ON THE NERVES OF THE LUNGS OF THE NEWT. By  
WILLIAM STIRLING, M.D., Sc.D., *Professor of the Institutes  
of Medicine (Physiology) in the University of Aberdeen.*  
(PLATES III. and IV.)

THE lungs of the newt consist of two long, narrow, transparent membranous sacs which extend far backwards into the abdominal cavity above the other viscera. Their outer surface is smooth and polished, as it is covered by a layer of the peritoneum lining the pleuro-peritoneal cavity. Each lung is a perfectly simple sac, with no septa or partitions on its internal surface, so that no depressions are seen on its outer aspect. The cavity of each lung communicates with the glottis by means of a narrow membranous tube which represents the trachea.

The peritoneal investment forms, as it were, a mesentery for each lung, but this mesentery exists only along the inner margin of the upper half of each lung, so that the lower or posterior half is quite mobile. The outer surface of the lung is covered by a layer of polygonal squames, whose existence is easily revealed by the action of silver nitrate. I have not succeeded in finding any stomata amongst the epithelial cells. A small quantity of white fibrous tissue, containing a meshwork of elastic fibres, lies immediately under the epithelial investment. A few branched pigment cells are found in this serous coat, especially along the course of the pulmonary vein. Indeed, the lung is very slightly pigmented, so that—as has long been known—it affords an excellent object for the microscopic examination of the circulation of the blood.

In considering the layers of tissue which form the wall of the lung, and proceeding from without inwards, in addition to those already mentioned—epithelium, fibrous tissue containing an elastic network—we come upon a layer of *non-striated muscle*. This forms a complete investment for each lung, and the muscular fibres are disposed circularly, forming a layer of nearly uniform thickness. The muscular fibres composing this layer are large and fusiform in shape, and have a well-defined nucleus containing an intranuclear plexus of fibrils, and they exactly resemble similar fibres which Klein and others have described

as occurring in the mesentery of the newt. Their characters are easily demonstrated by the usual method with ammonium chromate and subsequent staining with picro-carmin.

Inside this layer of non-striped muscle we come upon the layer of the blood-vessels, and internal to this the epithelium lining the lung. The nerves of the lung are distributed with special reference to the muscular coat.

Let us consider the arrangement of the blood-vessels.

*The Blood-Vessels.*—A branch of the pulmonary artery—narrower than the corresponding vein—runs to each lung. It enters the wall of the lung at its anterior end, and courses along the outer wall of the lung, and as it does so it gives off on each side a series of branches which form more or less acute angles with the main trunk. These branches run more or less horizontally round the lung towards the pulmonary vein, which lies along the opposite or inner side of the lung. They give off branches which rapidly split up into capillaries, from which the venous radicles arise usually midway between two branches of the pulmonary artery, and run nearly horizontally to join the large pulmonary vein. The arrangement of the pulmonary artery and vein is well shown in PL. III. fig. 1, which represents part of the lung of a newt laid open, and spread out flat, after it had been acted on in the usual way with gold chloride. P.A. represents the pulmonary artery, and P.V. the pulmonary vein. The branches of the P.A. are less regular than they usually are. The venous radicles arising between two branches of P.A. and their union at right angles with P.V. are well shown. In the same drawing the muscular layer is represented. It is not necessary to inject the blood-vessels of the lungs to see this arrangement, as their distribution is easily made out from the arrangement of the coloured blood-corpuscles.

There is an important point of difference, however, in the position of the pulmonary artery and vein with regard to the muscular coat. The large trunk of the pulmonary vein lies quite superficially—it is covered by the serous coat, and lies external to the muscular layer, whilst the pulmonary artery lies internal to the muscular layer. The capillaries in the pulmonary walls lie internal to the muscular layer and immediately under the epithelium lining the lung.

*The Pulmonary Epithelium.*—If a newt be killed by pithing, not by ether or chloroform, and one of its lungs be excised, slit open, and placed on a slide with its inner surface uppermost, and if it be moistened with a drop of salt solution, and examined with a low magnifying power, one can easily make out the arrangement of the ciliated epithelium which covers certain parts of the inner surface of the lung. The ciliated epithelium of the short trachea is prolonged into the lung in a special tract, viz., along the course of the pulmonary vein and its chief branches. The ciliated epithelium in its distribution follows closely the pulmonary vein, but it extends for a short distance beyond the margin of the vein and its large branches, where the ordinary epithelium lining the rest of the lung begins. The limits of distribution are abrupt and well marked, as is easily shown by the action of osmic acid, which fixes the tissue elements. No ciliated epithelium exists over the course of the pulmonary artery, which has internal to it, next the cavity of the lung, a plexus of capillaries, and the ordinary epithelium lining the lung. The pulmonary vein, on the other hand, lying as it does outside the muscular coat, has internal to it the muscular coat, no capillaries, but a layer of ciliated epithelial cells. Dr Thomas Williams<sup>1</sup> figures ciliated epithelium as occurring over the course of the pulmonary artery, where I have satisfied myself it does not occur. Williams also states that it occurs over the course "of the principal blood-vessels."

The epithelium lining the rest of the lung is easily seen. Nuclei in groups of two, three, or more, are recognised lying in the meshes of the capillary plexus lining the lung. These nuclei belong to the epithelial cells lining the lung. It is necessary to isolate these cells to ascertain their shape. This is best done by "dilute alcohol" as was first pointed out by Ranvier. When isolated they are seen to consist of a flattened cell plate enclosing a large globular nucleus, which can be shown to contain an intranuclear plexus of fibrils such as occurs in all cell nuclei. The arrangement of these cells is curious. They lie in groups, and the large nuclei occupy the small spaces left between the blood-capillary network, whilst the thin plates of

<sup>1</sup> "Respiration," by Thomas Williams (Todd & Bowman's *Cyclopædia of Anat. and Phys.* vol. v.).



each cell cover the capillary wall. The result of this arrangement is that the air in the lung is separated from the blood-stream merely by the cell plate of these cells and the squames which form the capillary wall. Ranvier<sup>1</sup> has shown that a similar arrangement exists in the lung of the frog, where the nuclei, with a small quantity of protoplasm surrounding them, lie in groups of two or three, and occupy the "inter-vascular fossæ," whilst the very thin cell plate extends over the capillaries. Ranvier gives a figure of lamellæ of such epithelium isolated by means of iodised serum. I have attempted to show the relation of these epithelial cells in the schema (Pl. IV. figs. 7 and 8). In fig. 7 c.c. represent the course of the capillary blood-stream, and s.s. represent the groups of epithelium seen from above. The scheme shows an ideal plan of the wall of the lung, where layer after layer is removed from the surface inwards. In fig. 8 an ideal vertical section of the wall of the lung is shown; c.c. again represent the blood-stream, while s.s. represent the epithelial cells, with their cell plates projecting over the capillaries.

*The Nerves.*—The nerves in the pulmonary wall are easily demonstrated by the modification of the gold chloride method, introduced by Ranvier. The medullated nerve fibres are easily revealed by the action of osmic acid. The gold chloride method which I have found to give the best results is Ranvier's<sup>2</sup> lemon-juice and formic acid method,<sup>3</sup> which he introduced for studying the terminations of the nerves in the œsophagus. The formic acid removes all the epithelium, so that a better view is obtained of the structures in the wall of the lung.

The chief pulmonary branches of the vagus enter each lung at its anterior extremity in two, three, or more strands, close to the pulmonary vein; a few non-medullated fibres follow the course of the pulmonary artery. The branches around the pulmonary vein are of unequal thickness, and during the whole of their course in the wall of the lung the large mixed nerve strands lie in close relation with the main trunk of the pulmonary vein. After they

<sup>1</sup> L. Ranvier, *Traité technique d'histologie*, p. 238, 1875.

<sup>2</sup> *Leçons d'Anat. générale (appareil nerveux terminal)*, L. Ranvier, p. 368, 1880.

<sup>3</sup> *Text-Book of Practical Histology*, W. Stirling, p. xlv., 1881.

enter the wall of the lung they exchange filaments, so that in this way a plexus is formed more or less dense along and around the course of the pulmonary vein and its larger branches. This arrangement is shown in Pl. III. fig. 1, which is taken from a preparation made by the gold chloride method above mentioned. The branches of the nerves are represented black (N.N.), and their relations to the course of the pulmonary vein are easily made out. As the vein lies superficial, *i.e.*, external to the muscular coat of the lung, the nerve fibres are also superficial to the muscular coat. They lie between it and the serous coat, so that the branches of the nerves therefore lie in direct relation with the muscular coat. The nerve fibres are medullated and non-medullated, so that the nerve strands are *mixed* nerve strands; and, in addition, a large number of nerve cells are found lying amongst the nerve fibres. The nerve cells are found along the whole course of these mixed nerve strands, where these are in relation with the pulmonary vein, and occur singly or in groups, and are usually most numerous where branches are given off. One may regard these groups of nerve cells as the homologues of the nerve ganglia, which have been described (Remak, Klein, Stirling) on the pulmonary branches of the vagus in mammals, and in frogs by Arnold, Beale, and more recently by Egerow and Kandarazki. The general appearance and arrangement of these nerve cells and their relations to the nerve fibres is well shown in Pl. III. fig. 2, which represents a small portion of the lower part of fig. 1 more highly magnified. The nerve strands (N.N.) as they pass towards the posterior extremity of the lung, give off branches which may contain one or more medullated fibres and several non-medullated nerve fibres, whose probable mode of termination I shall mention presently.

As already indicated, both medullated and non-medullated nerve fibres enter the lungs, and in a preparation which has been subjected to the action of osmic acid, it is easy to count the number of medullated nerve fibres, for, as is well known, the myeline is blackened by this reagent. I have counted as many as twenty of these fibres in the various mixed nerve strands, where they enter the lung. It is not so easy to make out the number of non-medullated fibres, but they are more numerous than the others. On tracing the course of the medullated fibres

one may find that a fibre which enters the lung in one nerve strand may leave this strand and enter a neighbouring one, so that there is an exchange of fibres between the different nerve strands. This exchange is represented in fig. 2. These medullated fibres leave the nerve strands either singly or in company with several non-medullated fibres, thus forming minute branches which run on to the muscular coat of the lung, where their mode of termination will be considered afterwards. These nerve fibres are slightly smaller than the corresponding fibres in the lung of the frog. It is obvious, therefore, that the size of the large mixed nerve strands becomes less and less as they are traced on towards the posterior end of the lung, although even there three or four medullated fibres may be found. On tracing medullated fibres onwards, one may find a fibre leaving one of the main strands passing into a branch, and without forming connections with any structure re-enter one of the main nerve strands at a lower level. I do not find that the medullated fibres form connections with any of the numerous nerve cells lying in the course of the nerve strands. When a medullated fibre finally leaves one of the main nerve strands it runs outside and upon the muscular coat of the lung, where it divides—usually into two branches, and always at a node of Ranvier,—each branch being nearly as large as the original fibre. Several fibres may lie together in a sheath of Henle, but gradually the different fibres take different courses, so that one may find only a single fibre which loses its myeline, and the axis cylinder after a time splits up into bundles of fibrils, which unite with fibrils from other nerves, and form a wide-meshed nerve plexus of fibrils outside and upon the muscular layer of the lung. From this *primary plexus* a series of finer branches are given off, which form a *secondary narrow-meshed plexus* lying nearly in the same plane.

The non-medullated nerve fibres, as already indicated, are more numerous than the medullated, and in their course in the large mixed nerve strands on the pulmonary vein they form connections with some of the processes of the nerve cells lying in these strands. They may easily be traced into the various nerve branches that are given off by these nerve strands. Sometimes, however, a branch consisting entirely of non-medullated fibres may leave the mixed strand. The fibres are enclosed in a

nucleated sheath of connective tissue, and here and there single nerve cells may be found lying between the fibres, or between them and the sheath (fig. 4). The fibres may form a plexus at a short distance from the main trunk, as is shown in Pl. IV. fig. 4, which represents several branches of non-medullated fibres, leaving one of the large mixed strands. On tracing these nerve fibres, one finds that they frequently divide dichotomously, and ultimately they formed a wide-meshed plexus—*primary plexus*. At the points of division nuclei are frequently found (fig. 6), and these nuclei are often triradiate in shape, presenting a resemblance in shape to the cells of the cerebrum. In the course of the fibres where no division takes place, fusiform nuclei are frequently seen. This network lies superficial, *i.e.*, external to the muscular coat, where it gives off a series of finer branches, which reunite and form a *secondary* nerve plexus with finer meshes. A small part of these two plexuses is represented in Pl. IV. fig. 6, where the stronger lines represent the primary plexus of fibres or fibrils, and the finer lines lying underneath them the meshes of the secondary plexus. This plexus gives off excessively fine fibrils, which dip down between the muscular fibres, but I have not been able to trace a direct anatomical continuity between the nerve and muscular elements, such as Ranvier has established for the nerves in the intestine of the leech.

As already indicated, the nerve cells are numerous. They exist in the branches of the vagus before they enter the lungs, and their arrangement and distribution after the nerves enter the lung is shown in Pl. III. fig. 2, where P.V. represents the pulmonary vein, N.N. the mixed nerve strands with groups of nerve cells, variable in size and arrangement.

One can find groups of these nerve cells along the course of the main nerve strands in relation with the pulmonary vein almost to the posterior end of the lung. They are not confined to the main trunks, however, but recur singly or in small groups in the larger nerve branches. Sometimes the cells lie singly between the sheath of the nerve and the nerve fibres, whilst others lie crushed up as it were between the nerve fibres. As already remarked, they are most numerous where a nerve twig is given off from the main mixed strand. This is well shown in Pl. III. fig. 2.

In these small aggregations of nerve cells it is easy to trace the course of a medullated fibre sweeping clear through the mass, and forming no connections with the cells, whilst, occasionally, one may see a connection between a nerve cell process and one of the non-medullated fibres. This arrangement is shown in Pl. IV. fig. 5, which represents a small group of nerve cells. M. shows a medullated fibre, whilst the other fibres are non-medullated. The shape of the nerve cells seems to vary considerably; some appear to be bipolar, whilst others are polygonal and have several processes, one or more of which may become continuous with a non-medullated fibre (fig. 5). Each cell contains a relatively large nucleus usually placed excentrically, and the body of the cell may or may not be enclosed in a capsule. Other nucleated masses of protoplasm devoid of processes lie amongst the nerve fibres, and it is not clear what is their exact nature. Perhaps they may represent developing nerve cells.

Up to this time I have described the arrangement of those nerve fibres which enter the lung along the course of the pulmonary vein, but, as already indicated, the pulmonary artery has also a very few nerve fibres in relation with it, but, as far as I have made out, they are very few in number, and are non-medullated. This arrangement is shown in Pl. III. fig. 1, where P.A. marks the position of the pulmonary artery, and N.N. indicate the fine non-medullated fibres along its course, over which they form a wide-meshed plexus, with the long axis of the meshes in the line of the artery. These branches may be traced along the artery almost to its termination. A wide-meshed plexus of non-medullated nerve fibres exists in the arterial adventitia, from which branches proceed to form a narrower-meshed plexus which lies in the arterial muscular coat. The relations of the adventitia plexus to the artery are shown in Pl. III. fig. 3. I did not find any nerve cells amongst these nerve fibres, nor did I find similar well-defined plexuses in the wall of the pulmonary vein.

Dr Wladimir Egerow<sup>1</sup> has given a description of the nerves of the frog's lung, where he shows that a similar arrangement of

<sup>1</sup> "Über die nerven der Lungen," by W. Egerow, *Centralblatt, f. d. med. Wissensch.*, No. 18, 1879.

the nerves exists in that animal. I have also investigated the nerves of the frog's lung, and in the main my observations agree with those of Egerow and also with the more recent results of M. Kandarazki,<sup>1</sup> but I reserve the publication of these results until another occasion.

My investigations on the nerves of the lung of the newt, frog, cat, rabbit, dog, and other mammals, led me to consider the nervous apparatus of the lung from the point of view of its development and homologies. The lungs are developed from the alimentary canal in the form of small buds, and in their fully-developed condition they contain anatomical elements comparable to those existing in the wall of the digestive tract. In the intestinal tract—from cesophagus to rectum—there are two great nerve plexuses—Auerbach's between the muscular coats, and Meissner's in the sub-mucous coat. These plexuses, whatever other function they may have, are related, the one to the muscular coat proper, and the other, in part at least, to the muscularis mucosæ, *i.e.*, of the mucous coat. At present I am unable to say whether the muscular tissue found in the posterior wall of the trachea ("trachealis muscle") in the bronchial muscles, and between the air cells in mammals' lungs, and surrounding those of the newt and frog, are the homologues of the muscular coat of the intestine or the muscularis mucosæ, or both. But I think we may very fairly compare the nerve plexus in the wall of the newt's lung with one or both of the nerve plexuses which exist in the walls of the whole intestinal tract. The nerve fibres are undoubtedly both afferent and efferent in their functions, as experiment has shown, but the large number of nerve cells in the lung leads one to believe that they exercise some important function—and one yet unknown—on the lung. It remains for experiment to decide whether they are related to the non-striped muscle, or are concerned with regulating the nutrition of the lung, *i.e.*, exercise a so-called trophic influence. It is more probable, as already hinted, that they bear the same relation to the lung that the nerve cells of Auerbach's and Meissner's plexus bear to the structures in the walls of the intestine. Whatever their function may be, it can be no unimportant one, for their

<sup>1</sup> *Archiv. für Anat. u. Physiol.* 1881.

existence in the lungs of so many animals, clearly points to their importance.

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## EXPLANATION OF THE PLATES.

### PLATE III.

Fig. 1 shows a view of the outer surface of the lung of a newt slit open after preparation with gold chloride. P.V. represents the pulmonary vein, and P.A. the pulmonary artery, while N.N. represent the nerves which accompany these vessels ( $\times 30$ ).

Fig. 2 shows a small part of fig. 1, consisting of the pulmonary vein and its nerves more highly magnified. P.V. is pulmonary vein, and N.N. the nerves. The dark lines represent medullated nerve fibres, and the lighter single and finer lines the non-medullated nerve fibres. Groups of nerve cells are seen along the course of the nerve trunks and their larger branches (oc. 3, obj. 4).

Fig. 3 shows the plexus of non-medullated nerve fibres which accompanies and surrounds the (P.A.) pulmonary artery (oc. 3, obj. 4).

### PLATE IV.

Fig. 4. A branch consisting of non-medullated fibres from a mixed nerve strand accompanying (P.V.) the pulmonary vein. It shows the fibres splitting up into fibrils, with triradial nuclei at some of the points of division. The dark fibre is medullated (oc. 3, obj. 7).

Fig. 5. A small portion of a mixed nerve strand from the plexus on the pulmonary vein. M. a medullated fibre; the other fibres are non-medullated, with polygonal branched nerve cells amongst them (oc. 3, obj. 7).

Fig. 6. A small portion of the primary and secondary plexus of nerve fibrils on the muscular coat (oc. 3, obj. 8).

Fig. 7. Scheme of the wall of the lung, showing layer after layer removed from without inwards. 1. Serous coat—squamous epithelium. 2. Layer of elastic tissue imbedded in a small quantity of connective tissue. 3 and 4. Branches of nerves and the muscular layer. 5. Layer of capillaries, with groups of epithelial cells between them. In some places the cells have fallen out. c.c. represent the capillary blood stream, and s.s. the islands of epithelium.

Fig. 8. Scheme of a vertical section of the wall of the lung. c.c. represent capillaries, and s.s. the arrangement of the pulmonary epithelium.

PELVIMETRY. By J. G. GARSON, M.D., *Assistant in Museum,  
Royal College of Surgeons, England.* (PLATE V.)

PERHAPS next in importance to the form of the skull as indicating race characters may be placed that of the pelvis. But although anthropologists are agreed in the main as to which are the most important measurements of the former, the same cannot be said regarding the latter. Authors have, as a rule, been contented to recommend a number of measurements without calling special attention to those which are of importance as indicating the general outline or form of the pelvis. No better example of this could be found than the list of pelvic measurements recommended by Verneau in his very valuable and elaborate monograph on *Le Bassin dans les Sexes et dans les Races*,<sup>1</sup> where no less than fifty-five different measurements are enumerated, but without reference as to their relative importance. Great diversities exist amongst anthropologists also regarding the mode in which measurements are to be made, the same measurement being often taken in different ways or from different points. Thus, the length of the sacrum is taken by some as the vertical depth of the five sacral vertebræ, while others have included a sixth in the cases where the first coccygeal vertebra is ankylosed to the sacrum, or, in other words, define the measurement as the absolute depth of the vertebræ ankylosed together to form the sacrum, whether these be five or six in number. Again, the width between the crests of the ilia is sometimes measured as the distance between the inner margins of each crest, and sometimes as the maximum width between the outer lips. The need of having the pelvic measurements rearranged, and a more commodious system of measurement instituted than any of those which have been hitherto proposed, will probably be admitted by all who have given attention to the subject. In the first place, it is most desirable that a general understanding should be come to as to how and from what points the different measurements should be made. To arrive at this it is necessary that the merits of each method be discussed, and the reasons for and

<sup>1</sup> Ballière & Co., Paris, 1875.



against the adoption of various measurements pointed out. Those given by former observers should be adhered to as much as possible, since modifications which are not absolutely necessary are to be deprecated on account of impairing the usefulness of measurements already recorded. In the second place, it appears to me that the study of the characters of the pelvis would be greatly facilitated if the various measurements were grouped according to their importance. The measurements of primary importance are those which express the general outline of the form of the pelvis. They are comparatively few in number, and, if arranged in a practical manner, would probably be given in osteological catalogues and general descriptions of the skeleton in the same manner that the chief measurements of the skull are usually stated. This would undoubtedly advance our knowledge of the characters of the pelvis and of the osteology of different races. For those studying more in detail the characters of the pelvis, further measurements, which might be classified as of secondary and tertiary importance, would be available according to the minuteness required by the author.

The first step in the direction of remodelling the pelvic measurements as I have indicated was that taken by Professor Flower, when, in a paper entitled "On the Osteology and Affinities of the Natives of the Andaman Islands," communicated to the Anthropological Society of Great Britain,<sup>1</sup> he defined and arranged "the measurements which seemed most likely to give useful results in comparing pelves with one another." This list contains twenty-one different measurements, most of which correspond to those given by Verneau, while a few are either original or modifications of measurements adopted by Verneau and others. Even this selected list, I hope to be able to show, contains more measurements than is requisite to give an idea of the general form of the pelvis, there being several measurements in it which I think may be classed amongst those of secondary importance. As indicative, however, of the importance I attach to the paper as a whole, I may say that I have taken it as the basis for my dissertation on the present occasion, and have adhered as closely as possible to the measurements as therein defined. The majority of the measurements which I give corre-

<sup>1</sup> See *Journal of the Anthropological Institute*, Nov. 1879.

spond, therefore, to those of Verneau, since, as I have already said, Professor Flower has adhered as closely as possible to that work. In some instances I have been reluctantly obliged to recommend some modifications, chiefly in the mode of making the measurements, and in two or three cases I have proposed measurements not given either by Verneau or Flower.

On the present occasion I have attempted to tabulate only those measurements which I think are of primary importance from indicating the general form of the pelvis, and have arranged them in the order of sequence which I have found to be most convenient. This latter is somewhat different from that recommended by Professor Flower, but I may say has been arrived at only after a good deal of practice and consideration. The alteration may partially be accounted for by the fact that several measurements included in Professor Flower's list have been omitted in mine.

In order to facilitate the discussion of the question of measurements, and to assist us in forming a more correct estimate of their respective value than could otherwise be done, I have selected the average measurements of the pelves of 14 Europeans, 5 Australians, and 13 Andamanese females (all the material of those races and that sex at my disposal) on account of the well-known diversity in physical and cranial characters which these races present, and have appended a table of the same. As sexual differences in the form of the pelvis are considerable, each sex requires to be considered separately; the material at my disposal, in the case of the European and Australian males, being deficient, I have thought it advisable to postpone the consideration of these differences until I have been able to measure more pelves, when I hope also to discuss the individual differences in the female. With additional material it is probable that the averages I have given on the present occasion for the Australian females may require some modification. The value of indices to facilitate comparisons between measurements is universally recognised. As a rule it has been customary to have several standards of comparison for the various parts of the pelvis; thus a sacral index has been formed by comparing the length and breadth of the bone, an index of height by comparing the height and width of the pelvis, a pelvic index by comparing the

antero-posterior diameter of the brim with the transverse diameter of the same, and several other indices. In each case a different standard of comparison is taken. This admits only of comparisons being made between corresponding parts, but does not show the relation which the various measurements bear to one another. The breadth of the sacrum can be compared to the length of that bone in different pelves, but it cannot be compared to the transverse diameter of the brim, or to the total breadth of the pelvis, since different standards for comparing those breadths are taken. By adopting a single standard and comparing the several measurements with it, this obvious disadvantage can be removed, and a minute comparison between the various parts is practicable. The question will naturally be asked, Which measurement is to be taken as the standard? This is a matter of great importance, and one regarding which there may be several opinions. In making a selection, several things must be kept in mind. The standard should be a measurement which varies very little in different pelves of the same race; it should be one of the chief or fundamental measurements; it should be one easily made, and should, if possible, not be one taken between bony points influenced by muscular development. I have selected, provisionally at least, for reasons which I shall explain later on, the transverse diameter of the brim as the standard, and have formed indices from the average measurements as compared to it. Under the table of measurements will be found the indices of the three races thus formed. The instruments I have found most useful for pelvimetry are three in number:—1. An osteometer, an instrument similar to that used by shoemakers for measuring the length of the foot, but with metal uprights, the extremities of which terminate each in a fine point; 2. An instrument like that used by hatters to measure the internal diameters of hats; and 3. A goniometer for measuring angles. With the first the great majority of the measurements are made; the second, though not absolutely required, is very useful for taking the diameters of the pelvic cavity. The goniometer is made so as to measure angles less than two right angles, such as the sub-pubic angle, for which alone, in the measurements I have given, it is required; and as it has not been previously shown or described, I have figured it

(Pl. V.). All the measurements are stated in millimetres, this being undoubtedly the most convenient scale, and the most generally used for all anthropological measurements.

The following is a list of the measurements which my observations have led me to believe are the most important, and which I propose to discuss *seriatim*. I have found it convenient to use the initial letters of each measurement, both when measuring pelves and in heading the different columns in the table, and as some difficulty might be experienced in recognising for what they stand, each contraction used is given before its respective measurement in the list:—

Contractions  
used in Table.

*List of Measurements.*

- |          |     |   |
|----------|-----|---|
| S.L.     | 1.  | Sacral length. Vertical length of the 5 sacral vertebrae.                     |
| S.B.     | 2.  | Sacral breadth. Maximum breadth of 1st. sacral vertebrae.                     |
| A.S.S.W. | 3.  | Width between the anterior superior spines of the ilia.                       |
| C.W.     | 4.  | Width between the crests of the ilia.   |
| P.H.     | 5.  | Maximum length of the innominate bone or pelvic height.                       |
| I.B.     | 6.  | Maximum breadth of the ilium.   |
| P.S.S.W. | 7.  | Width between the posterior superior spines of the ilia.                      |
| A.S.W.   | 8.  | Width between the posterior margin of the acetabulum and the symphysis pubis. |
| P.I.D.   | 9.  | The pubo-ischeal depth.   |
| A.P.D.B. | 10. | The antero-posterior diameter of the brim.                                    |
| T.D.B.   | 11. | The transverse diameter of the brim.  |
| A.P.D.O. | 12. | Antero-posterior diameter of the outlet.                                      |
| T.D.O.   | 13. | Transverse diameter of the outlet.  |
| S.P.A.   | 14. | Sub-pubic angle.  |

*The Length of the Sacrum.*—This is measured from the centre of the upper margin of the promontory of the first sacral vertebra to the middle of the inferior border of the body fifth sacral. It does not vary much in actual length in the three races, but in proportion to the size of the pelvis it varies considerably, being proportionally longest in the Andamanese and shortest in the Europeans. The length of the sacrum determines the depth of the pelvic cavity posteriorly. This is therefore deepest in the Andamanese and shallowest in Europeans, while in the Australians its depth is intermediate between that of the Europeans and Andamanese.

*The Breadth of the Sacrum.*—The maximum breadth of the first sacral vertebra, which may be across the anterior, middle or posterior part of its upper surface. Although the actual breadth of the sacrum varies considerably in the three races, yet it is much more proportional to the transverse diameter of the brim than the length of the bone. In the Andamanese it is somewhat broader than in the Europeans or Australians, indicating that the iliac bones are widest apart in that race: of this notice will be taken subsequently.

Professor Flower has given in his paper the breadth of the third sacral vertebra as well as that of the first. The chief information to be gained from this measurement is probably an idea of the narrowing of the pelvic basin towards the outlet by the downward, inward, and backward slope of the iliac bones. This is, I think, sufficiently shown by the measurement of the transverse diameter of the outlet; the width of the third sacral vertebra may therefore be omitted as one of primary importance, though perhaps very useful in minute researches on the form of this part of the pelvis.

*The Width between the Anterior Superior Spines of the Iliæ.*—The manner in which this measurement has been taken by anthropologists varies. This is probably due to the difficulty of defining an exact point on each spine from which to measure, while Verneau measures between the internal margins of the spines, Professor Flower recommends that the measurement should be made from the centre of the most prominent part of one spine to the corresponding point on the other. My observations are entirely confirmatory of this latter method of measuring. That the measurement is important as a race character may be seen by comparing its average length and index to the transverse diameter of the brim in the three races, as has been done below:—

	A.S.S.W.	T.D.B.	Index.
European, ♀, . .	231 mm.	133 mm.	173·8
Australian, ♀, . .	198 mm.	118 mm.	167·8
Andamanese, ♀, .	172 mm.	103 mm.	167

*Width between the Iliac Crests.*—This measurement, like the last, is taken in different ways, but is, I think, best indicated by

the greatest width between the external surfaces of the crests, as defined by Professor Flower. A measurement from the inner lip of one crest to that of the other cannot be made so accurately as that taken in the manner just described, on account of the inner lip being rounded, and offering no well-defined margin from which to measure, though it is true that the other is open to the objection that it varies somewhat according to the muscular development of the person and the consequent ruggedness of the bones.

The measurement is of importance, as showing the outline of the iliac crest when considered in relation to the transverse measurements of the pelvis between the anterior and posterior superior spines of these bones respectively. As a rule, the greatest inter-crest width is situated between the middle thirds of the iliac bones, in a line with the promontory of the sacrum. The length of this measurement and its index, compared to the transverse diameter of the brim, in the three races is as follows:—

	C.W.	T.D.B.	Index.
European, ♀, . .	271 mm.	133	203·8
Australian, ♀, . .	240·6 mm.	118	204·1
Andamanese, ♀, . .	207·7 mm.	103	201·9

It will be seen that the crest width is proportional to the size of the pelvis, the differences in the indices of the three races being very small. Regarding the part it plays, in indicating the form of the iliac crests, I shall have occasion to refer subsequently.

*The Pelvic Height*, or the height of the innominate bone.—This is measured from the highest part of the iliac crest to the lowest part of the tuber ischii. The table and index show that there are well-marked race differences between the height of the pelvis in the Europeans, Australians, and Andamanese, so that the measurement is important. I understand that it is considered by French anthropologists to be one of the most distinctive pelvic measurements, on account of its index forming well-marked sexual and race differences when compared to the breadth of the brim or inter-crest width. In the European the pelvis is lowest in proportion to its transverse diameter and highest in the Andamanese.

Professor Flower has placed in his list the height of the ilium as a useful measurement. He directs it to be taken from the "cotylon," or point on the posterior rim of the acetabular cup where the ilium and ischium join, to the summit of the iliac crest. Owing to the former point being difficult to define with accuracy after the bones forming the innominate have become fused together, I do not think great reliance can be placed upon the measurement. I therefore do not think it should be included in a list of measurements of chief importance. A measurement taken from the point in the bottom of the cup, where the three component parts of the bone unite to the summit of the crest, would probably be a better criterion of the height of the ilium, but the difficulty of defining the acetabular point exactly militates against its accuracy. As far as my observations go, the height of the ilium is proportional to the height of the innominate, as may be seen in comparing these measurements and indices in the Europeans, the Australians, and Andamanese females—

	P.H.	P.H. Index.	I.H.	I.H. Index.
European, ♀, . . .	202	151·8	124	93·4
Australian, ♀, . . .	184	155·9	116	98·3
Andamanese, ♀, . . .	167	162·1	102·6	108·0

*The Breadth of the Ilium.*—The maximum breadth of the ilium measured from the anterior superior spine to the posterior superior spine across the bone. The three races under consideration show very little proportional variation in this measurement compared to the size of the transverse diameter, though the actual variation in breadth of the bone is considerable. This is well illustrated in the table.

After reading a paper by Broca, directing attention to the form of the scapula as a race character, and tabulating a scapular index, the idea of forming a similar index in the case of the innominate bone occurred to me. Accordingly, I measured all the innominate bones of different races at my disposal in the Museum of the Royal College of Surgeons of England. The results I obtained, however, are by no means so satisfactory as those derived from measurements of the scapula. Broca took

the length of the scapula as 100, and formed a scapular index thus:  $\frac{\text{Breadth} \times 100}{\text{Length}} = \text{index}$ . In a similar manner I, by taking the length of the innominate as 100, formed an innominate index thus:  $\frac{\text{Breadth} \times 100}{\text{Length}} = \text{index}$ .

The following are some of the indices I obtained in different races:—

Number of Innominate Bones.	Race.	Sex.	Index.	Number of Innominate Bones.	Race.	Sex.	Index.
11	Andamanese, . .	♂	71·55	14	Andamanese, . .	♀	73·99
2	New Caledonian,	♂	71·73	...	...	...	...
2	Savage Islanders,	♂	71·90	...	...	...	...
2	Peruvian, . . .	♂	71·99	4	Peruvian, . . .	♀	74·01
4	Egyptian, . . .	♂	72·29	...	...	...	...
6	Negroes, . . .	♂	72·82	...	...	...	...
2	Tahitian, . . .	♂	73·39	...	...	...	...
4	Eskimo, . . .	♂	73·48	2	Eskimo, . . .	♀	76·39
2	Papuan, . . .	♂	73·78	...	...	...	...
38	European, . . .	♂	73·79	33	European, . . .	♀	77·57
4	Canary Islanders,	♂	73·92	...	...	...	...
4	Tasmanians, . .	♂	74·31	2	Tasmanian, . .	♀	83·91
9	Australians, . .	♂	75·40	10	Australian, . .	♀	77·45

From the above table it will be seen that several races allied in general as well as anatomical characters are widely separated from each other.

*The Width between the Posterior Superior Iliac Spines.*—This measurement is usually taken, and has been recommended by Professor Flower to be taken, between the two points of the ilia which approach nearest to one another behind the sacrum wherever that may be. After using this mode of measuring for some time I became dissatisfied with it, as frequently I found that the nearest points of the ilia were eminences of bone formed by the ossification of some of the ligamentous or tendinous structures which abound at that part of the pelvis for the union of the sacrum to the ilia and the origin of the erector spinæ muscle, and because the point was not a certain one, being sometimes situated near the posterior surface of the sacrum and sometimes at the most posterior extremity of the spine. A much more satisfactory measurement, and one which corresponds to that between the anterior extremity of the iliac crest, can be



made by measuring from the centre of the most prominent part of one posterior superior spine to a similar point on the other. These points are also the posterior points of greatest transverse breadth of the ilia, and can readily be found. Verneau has unfortunately defined this measurement as given by him so indefinitely that I am unable from his description to determine the precise points between which he measures. The results of his measurements of European pelves so nearly correspond to those given by Professor Flower that I believe he measures the width in almost the same manner.

The width between the ilia posteriorly is important as indicating, when compared with the width between the anterior superior spines, the backward and inward slopes at which the innominate bones are placed, and the consequent narrowing of the pelvis behind. On this account, it is desirable that the two measurements should be taken in a similar manner, as I have proposed, so that they may be strictly comparable. The degree of narrowing of the pelvis in different races seems to be a well-marked character. In the Europeans the innominate bones slope inwards and backwards much more rapidly than in the Andamanese, that is to say, the pelvis of the European is narrow behind in proportion to its size than in the Andamanese. A glance at a typical pelvis of each of these races (Pl. V.) will at once confirm the truth of this statement.

The greater proportional narrowing of the pelvis posteriorly in the Europeans is also indicated by the breadth of the sacrum, which as we have already seen, is narrower in that race than in the Andamanese.

The average measurements and indices of the posterior interiliac width as measured by the plan usually adopted compared to the distance between the centres of spines, as I have suggested, are as follows:—

			Indices.	
	Old Method.	New Method.	Old Method.	New Method.
European, ♀	72 mm.	83 mm.	55	63·1
Australian, ♀	69 mm.	85 mm.	58·5	72
Andamanese, ♀	64 mm.	79 mm.	62·1	76·7

With the measurements between the anterior superior spines, the crests and the posterior superior spines of the ilia, a precise idea of the outline of the so-called false pelvis can be formed. The importance of these three measurements as race characters, can be well seen by comparing them in the Europeans, Australians and Andamanese.

Race.	Sex.	A.S.S.W.	C.W.	P.S.S.W.	Indices.		
					A.S.S.W. Ind.	C.W. Ind.	P.S.S.W. Ind.
European, .	♀	231·5	271	84	173·8	203·8	63·1
Australian, .	♀	198·4	240·6	85	168	204·1	72
Andamanese, .	♀	172·1	207·7	79	167	202	76·7

From this table it will be seen that the width of the pelvis between the crests remains very constant in the three races in proportion to the transverse diameter of the brim. The other two measurements show a reversed relation in the three races. In the Europeans the anterior spines are wide apart, the anterior portion of the false pelvis is therefore also widest in them, whereas the posterior portion is narrower than in the Australians, and still more so in the Andamanese. In the Andamanese the anterior part of the false pelvis is narrowest, the distance between the spines being shortest, whereas the posterior part is considerably broadest, the posterior spines being wider apart than in the European or Australians. The iliac bones are therefore more nearly parallel than in the other two races. The Australians differ little from the Andamanese in the distance between the anterior spines, but hold in an intermediate position between the Europeans on the one hand and the Andamanese on the other, both in respect to this measurement and to the width between the spines posteriorly. The crest width would seem from this to be, as it were, the centre upon which the bones are revolved.

*The Acetabulo-Symphysial Width.*—The distance between the most postero-external part of the rim of the cotyloid cup to the symphysis. Professor Flower has defined the measurement as the distance between the cotylo-n, or the point on the rim of the acetabular cup where the ilium and ischium join, and the symphysis. The difficulty of determining this point with precision in

the adult pelvis, where frequently all traces of the union are lost, is, in my opinion, a very strong reason against its being taken as a point of measurement. The point, as I have just defined it, is preferable also, because it gives the extreme width of each half of the pelvis from the mesial line, as in viewing the pelvis from its anterior aspect—the chief object of the measurement. The difference between the measurement as thus defined and that between the symphysis and cotylon is at most 1 millimetre, since the most external point of the cotyloid cup is often at the cotylon, though most frequently a few millimetres above it. The difference between the two measurements of this width is almost entirely one of definition.

The measurement presents a certain amount of variation in the three races; in the Europeans it is shortest, and longest in the Andamanese, while the Australians are intermediate, though they approach nearest the Europeans. It is probably an open question whether the measurement might not be superseded by a measurement of the pubic bone from its apex in the cotyloid cup to the symphysis. As yet I have not been able to define accurately the point in the cup where the three bones forming the innominate unite.

*Pubo-Ischiatic Depth.*—The distance between the upper surface of the pubis and the lower surface of the ischium. The points from which I propose that the measurement should be made are the smooth level surface on the pubic side of the ilio-pectineal suture above, to the lowest part of the tuber ischii. Verneau defines a measurement very similar to this in his work on the pelvis, but he takes as his superior point the ilio-pectineal eminence. To me it appears that this point is objectionable, from its being subject to considerable variation in height in different individuals, often ill-defined and situated on a sloping surface, somewhat too near the acetabulum to indicate the depth of the antero-lateral aspect of the pelvis. The measurement, as I have defined it, can be very easily and certainly taken as the upright or projecting arm of the osteometer rests naturally on that portion of the bone. The results obtained from it, taken as here recommended, are as follows:—In the Europeans it is 91·4 mm., in the Australians 82·2 mm., and in the Andamanese 76·4 mm., giving an average index in the three races compared

to the transverse diameter of the pelvis of 68·5 in the Europeans, 69·5 in the Australians, and 73·8 in the Andamanese. Verneau gives as the average of the measurement, according to his method, 93 mm. in the Europeans and 91 in the Australians, which give indices, when compared to the average transverse diameter of the brim in the skeletons of these races measured by me, of 71 and 77·1 respectively. The chief value of this measurement is that it indicates the depth of the anterior part of the pelvic cavity in the same way that the length of the sacrum does the posterior part. If these two measurements are compared it will be seen that they agree.

				Indices.	
Race.	Sex.	S.L.	P.I.D.	S.L.	P.I.D.
European, . .	♀	101	91·4	75·2	68·5
Australian, .	♀	91·4	82·2	77·1	69·5
Andamanese, .	♀	91·4	76·4	88·8	78·8

In the Andamanese the pelvic cavity is deeper than in the Europeans and Australians both in front and behind, as is indicated by the pubo-ischiatic depth and the length of the sacrum. The Australians, it appears, differ little from the Europeans in this respect, that is to say, if the results obtained from the small number of pelves measured give a true average of the character of the race.

*The Antero-posterior and Transverse Diameters of the Brim.*—Probably less diversity of opinion exists regarding the great importance and the method of making these measurements than almost any others. Upon these diameters the whole pelvis seems to be constructed, and they determine the variations in the other measurements of a similar nature. The antero-posterior diameter of the brim is the distance between the anterior superior margin of the promontory of the sacrum, situated on the body of the first sacral vertebra, and the most adjacent point of the symphysis pubis, or the minimum distance between the sacrum and the symphysis pubis. The transverse diameter of the brim is its maximum width measured at right angles to the line of antero-posterior diameter. This is usually situated a little above

and anterior to the top of the great sacro-sciatic notch, and corresponds very closely to the line of greatest crest-width. These two diameters, and indeed all other measurements within the cavity of the pelvis, can be very conveniently and accurately made with the second instrument described.

The average antero-posterior diameter of the brim in the female pelvis measured by myself is 106·6 mm. in fourteen Europeans, 108·6 mm. in five Australians, and 99·1 mm. in thirteen Andamanese. Verneau, in his work on the pelvis, already referred to, gives the average of this measurement in thirty-five European female pelvis measured by him as 106 mm., and in two Australian females 100 mm. The similarity of the results obtained by Verneau and myself in the case of the Europeans is noteworthy; the discrepancy in the case of the Australians can easily be explained, as I find that of the five measured by me, two are respectively 99 and 100 mm., while one is as high as 127 mm. More skeletons require to be measured before the average antero-posterior diameter in the case of the Australians can be determined. The variations from the averages above given of this measurement in the Europeans and Andamanese is comparatively little. In the Europeans there is only a single instance in which antero-posterior diameter is 13 mm. below the average, and the greatest variation above the average is 9 mm. Over the whole fourteen pelvis the mean variation is 2·8 mm. (see Table I.), on either side of the average (106·6). The variation in the Australians in one instance is 18 mm., above the average (109 mm.), and in another 10 mm. below the average, over the five pelvis. The mean variation is 4·4 mm. The variation in the Andamanese in one pelvis is 18 mm. above the average (99 mm.), while one is 16 mm. below it. The mean variation in the thirteen pelvis is 3·7 mm. Over the three races together the mean variation of the antero-posterior diameter is 3·6 mm. The average transverse diameter of the brim in the fourteen European female pelvis given in Table II. is 133 mm. The maximum diameter attained by any pelvis is 146, or 13 mm. above the average, while the minimum is 117, or 16 mm. below the average. Verneau found that, in thirty-five female European pelvis measured by him, this diameter was 135 mm. The averages obtained by Verneau and myself agree in the main;

that given by Verneau is probably the more correct.<sup>1</sup> In the five Australian pelvis measured by myself, it is 118·2 mm., and in two measured by Verneau 125 mm. The discrepancy between those numbers is considerable, but from so few pelvis no deductions can be drawn. One of the pelvis in the College of Surgeons' Museum measures 125 mm., another 131, or 7 and 13 mm. above the average respectively, while the others vary from 109 (9 mm. below the average) to 113 mm. In the Andamanese pelvis, the average breadth of the brim is 102·8 mm.; the broadest measures 110 mm., or 7 mm. above the average, while the narrowest is 95 mm., or 8 mm. below the average. By reference to Table I. it will be seen that over the whole fourteen European pelvis, the mean variation from the average transverse diameter is 2·9 mm. In the Australians, the mean variation of the five pelvis is 3·9 mm., while in the thirteen Andamanese it is 1·5 mm. Over the whole three races together the mean variation of the transverse diameter of the brim is 2·8 mm.

By incorporating the measurements of the antero-posterior and transverse diameters given by Verneau with those derived from pelvis measured by myself, we get an average antero-posterior diameter in forty-nine Europeans of 106, and in seven Australians of 108·6, and a transverse diameter of 134·5 in the Europeans, and of 120·1 in the Australians. As the individual measurements of the pelvis measured by Verneau are not given, a more detailed examination of them cannot be made.

The antero-posterior and transverse diameters of the brim, being as it were the basis upon which the whole pelvis is constructed, and being subject as we have seen to comparatively slight variation in different races, it appears to me that one or other of these measurements is pre-eminently eligible as a standard to which to compare other measurements by forming an index with them. As already stated, I have selected the transverse diameter as the standard, and have formed an index of each measurement as compared with it, (which is taken as 100) by multiplying the respective measurements of each pelvis by

<sup>1</sup> The slight discrepancy between the averages given by Verneau and myself appears to be due to there being a greater allowance made for the thickness of fibro-cartilage of the symphysis in the pelvis in the Paris collection than in those in the Royal College of Surgeons' Museum.

100, and dividing the various products by the transverse diameter of the brim of its own pelvis, thus—

$$\frac{\text{Crest width} \times 100}{\text{Trans. diam. of brim,}} = \text{index} \quad \frac{\text{Pelvic height} \times 100}{\text{Trans. diam. of brim,}} = \text{index, \&c.}$$

The reasons which led me to adopt the transverse diameter as the standard in preference to the antero-posterior diameter are chiefly the following:—The former can be measured quite as easily and exactly as the latter, especially if the second instrument described be used for the purpose. The mean variation of the transverse diameter is less in the Australian and Andamanese than that of the transverse diameter, while in the Europeans the variations of both are equal. Over the three races considered together, the mean variation of the transverse diameter of the brim is 2·8 mm., while that of its antero-posterior diameter 3·6 mm. The slight advantage the transverse diameter has in being more constant than that of the antero-posterior diameter would of itself give the former a prior claim to be selected as the standard. The majority of the pelvic measurements happen to be transverse ones; and it is easier to convey an idea to the mind regarding the relative length of any of them by using as the standard a measurement of a similar nature. One very frequently also requires to compare the height of the pelvis, or of different portions of it, with the breadth; it is therefore very useful to have as a standard a transverse measurement. But perhaps the strongest argument for the adoption of the transverse diameter of the brim as the standard of comparison is that it has already been almost universally taken by anthropologists and others as the standard to which the antero-posterior diameter has always been compared; and by adopting the latter as the standard, the results of previous observations would, to a certain extent, be impaired in usefulness, not to speak of the confusion which would be occasioned by the change, consequences much to be deprecated at present when the material at our disposal is so deficient in quantity. I am aware that it will be urged by some that the standard should be a measurement in the median line of the body, and that the antero-posterior diameter is the more distinctive measurement. The question is undoubtedly an open one, and one upon which it is very

desirable to have the opinions of various anthropologists of practical experience. Though for the present I have taken the transverse diameter of the brim as the standard of comparison, if further observations on man and the other mammalia show that by taking the antero-posterior diameter as the standard, any advantage is to be gained which is not attainable while the transverse diameter is used, I shall abandon the latter for the former.

The transverse diameter of the pelvis between the acetabula has been proposed by Professor Flower, who defines the measurement as the "distance between the posterior margins of the two acetabula at the junction of the ilium and ischium; the point for convenience called *cotylon*." He has also proposed another between the most internal parts of the cotyloid cavities, or the width between the bottoms of the cups. Those measurements are situated somewhat in front of the transverse diameter of the brim, but a glance at the pelvis will suffice to show that they are all widths of nearly the same part. An interesting question, therefore, is the relation that they bear to one another, and its answer will determine whether they are all necessary for indicating the form of the pelvis at that particular point. The first, which is called the inter-acetabular width, in contradistinction to the inter-cup width, is measured between points which, I have had already occasion to show in discussing the height of the ilium, are very difficult to define accurately in the adult pelvis, and which are liable to vary according to the degree of muscular development of the individual; they are also situated on surfaces, which slope downwards and inwards, as can be readily seen if the pelvis is viewed from before, and do not indicate the greatest width between the acetabula. The inter-cup width seems to be a more satisfactory measurement, and would be so if it could be more accurately defined, or if it could be made between the point of junction of the three component parts of the innominate bone of the one side to the same point on the other side. The form of the bottoms of the acetabular cups varies in different pelvises; in some the most internal points of the concavities are flat. In those the measurement can be very easily and certainly made; but unfortunately there are other pelvises, in which the deepest part of the cup slopes directly into the cotyloid notch, when it



is very difficult to ascertain the true bottom, and therefore to get the exact measurement desired. Another objection to this measurement, though one which is disappearing as better modes of mounting skeletons are being introduced, is that it is often impossible to reach the deepest points of the cups owing to the heads of the femurs being unattachably articulated with the acetabula. The transverse diameter of the brim, on the other hand, is, as has been stated, one of the easiest measurements of the pelvis, and can be made with great accuracy in all cases. Unless, therefore, some decided character can be elicited by means of the acetabular measurements, I think they cannot be included in a list of measurements of primary importance. The relation they bear to one another is best indicated by comparing their averages side by side in the three races under consideration :—

	I.A.W.	T.D.B.	I.C.W.	Amount I.A.W. is greater than T.D.B.	Amount I.C.W. is less than T.D.B.
14 European, ♀ .	190	183	115	57	19
5 Australian, ♀ .	166	118	101	48	17
13 Andamanese, ♀ .	150	103	90	47	13

Taking the transverse diameter of the brim of each race as 100, the following table shows the proportions which the inter-acetabular and inter-cup width bear to it :—

	I.A.W.	T.D.B.	I.C.W.
Europeans, . .	142·9	100	86·4
Australians, . .	140·7	100	85·6
Andamanese, .	145·6	100	87·3 ;

The inter-acetabular width is 57 mm. greater, and the inter-cup width 19 mm. less, than the transverse diameter of the brim in the Europeans, the Australians, and Andamanese; the first is greater than the second by 48 and 47 mm. respectively, while the third is less than the second by 17 and 13 mm. respectively. The differences between the indices correspond very closely to those of the measurements. Thus far nothing

has been found to justify the retention of the inter-acetabular or inter-cup widths in the list of measurements of primary importance, and, as I believe, the transverse diameter of the brim expresses all that can be learned from those two measurements, I have omitted them.

The oblique diameters of the pelvic brim have had considerable importance attached to them by Verneau. Flower, however, omits them altogether. The result of my observations convinces me that they cannot be considered of such importance as to warrant their introduction into a list containing only the measurements necessary to indicate the general form of the pelvis, though, as accessory measurements, they will probably be found useful.

The form of the pelvic outlet, like that of the brim, is indicated by two chief measurements,—the one antero-posterior, the other transverse. Besides these there are several accessory measurements which have been proposed by various anthropologists.

*The Antero-posterior Diameter of the Outlet.*—As representing this width Verneau has given two measurements,—one from the anterior and inferior margin of the last sacral vertebra to the lowest part of the symphysis pubis, and another from the tip of the last coccygeal vertebra to the same part of the symphysis. As the second appears to be a very uncertain measurement, on account of the coccyx being movable, in most instances, in the living subject, and frequently also in the skeleton, I prefer the first or sacro-symphysial diameter, but with a somewhat different definition. I would propose that it be defined as the width between the centre of the anterior-inferior margin of the body of the *fifth* sacral vertebra and the most adjacent point of the symphysis pubis. The average length of this measurement, as just defined, is in the Europeans 116 mm., in the Australians 107.6 mm., and in the Andamanese 100.6 mm.

These numbers give an average index with the transverse diameter of the brim in the Europeans of 87.2, in the Australians 91.5, and in the Andamanese of 98, thus indicating that there is a well-marked difference in this diameter between each of the three races.

*The Transverse Diameter of the Outlet.*—The maximum width of the pelvic outlet, measured at right angles to the antero-posterior diameters of the outlet, between the most widely

separated points, on lines passing parallel to the brim-line from the spines of the ischia to the lower ends of the obturator foramina. The lines referred to are indicated in most pelves by a small ridge, or rather an elevation of the surface of each ischium, best marked nearest the spine. The greatest width is situated as a rule nearer the spines than the obturator foramina. The measurement can be made very accurately, and easily, especially with the second measuring apparatus described. Another method of arriving at the points referred to is to hold the pelvis at arm's-length against a dark surface, with the outlet placed perpendicularly, by closing one eye, and looking from the outlet through the pelvis with the other, the greatest transverse diameter of the outlet can at once be seen and measured. In the European females this diameter measures on an average 116 mm., in the Australians 105 mm., and in the Andamanese 93 mm. The indices of these respective measurements, when compared with the average transverse diameter of the brim of the race to which each belongs, is in the Europeans 87, the Australians 89, and the Andamanese 90·2. Verneau has defined a transverse diameter of the pelvic outlet, but not with sufficient accuracy that it can be followed. There is no evidence that the measurement proposed by him corresponds to that just defined, as he states its average to be 137 mm. in thirty-five female European pelves, the maximum individual measurement being 155 and the minimum 120 mm., whereas the maximum of that proposed by me is 129 mm. and the minimum 106 mm.

The antero-posterior and transverse diameters of the pelvic outlet, when compared with the corresponding measurements of the brim, show the difference in size between the inlet and outlet of the pelvis. It will be observed that the antero-posterior diameter of the inlet is smaller than that of the outlet in the European, while in the Andamanese the reverse is the case; in Australians the two measurements are practically equal. Whereas on comparing the transverse diameters of the brim and outlet, openings, we find that the transverse diameter of the former is always greater than that of the latter, the proportion being in the three races respectively as 100 to 87, 89, and 90.

Those comparisons just made can be best illustrated by placing the measurements in the following tabular form:—

Races and Sex.	Inlet.		Outlet.		Indices of Inlet.		Indices of Outlet.	
	A.P.D.B.	T.D.B.	A.P.D.O.	T.D.O.	A.P.D.B.	T.D.B.	A.P.D.O.	T.D.O.
European, ♀	106·6	133	116	116	80	100	87	87
Australian, ♀	108·6	118·2	107·6	104·8	92	100	91	89
Andamanese, ♀	99	103	100·6	93	96	100	98	90

The transverse diameter of the outlet may be seen, by an examination of the general tables, to correspond closely to the breadth of the upper surface of the first sacral vertebra, but, as far as I can make out, the one has no relation to the other, and must be considered an incidental circumstance.

The spines of the ischium undoubtedly play an important part in the formation of the pelvic outlet, and it might naturally be supposed that the width of the outlet between them would be an important measurement. Unfortunately, they are so frequently broken that the measurement can seldom be obtained. Even if they were usually perfect, I am not certain that they would better enable us to make comparisons between the outlet of different pelves unless the ligaments were also present. The greater or less development of these spines depends upon the amount of development of the ligaments uniting the sacrum and ischium. This, therefore, causes the measurement to be one liable to vary a good deal, not only in different races, but in the same race. Its value, I think, is very questionable as one of primary importance, though it is very useful as an accessory measurement.

The width between the tuberosities of the ischia has been given by Verneau and Flower, who both measure it between the outer surfaces of the tuberosities. As its form and size are determined to some extent by the muscular development of the lower limbs, and as it cannot be considered a very satisfactory measurement, I think it must also be classed amongst the accessory measurements of this part of the pelvis. The transverse diameter of the outlet expresses all the information that can be derived from the inter-tuber ischial width regarding the breadth of the lower part of the pelvis.

*The Sub-pubic Angle* is formed by the meeting of the ischio-pubic rami of each side at the lower extremity of the symphysis.

It is measured with a goniometer constructed so that the apex of the instrument fits closely into the angle ; a projector is fixed to one arm of the instrument, while the other is free to travel through a space not greater than two right angles. Care must be taken to have the scale on the projector of moderate size and not too small, as then it is difficult to read off the angle, and very slight movement of the free arm causes considerable variation in the reading. The size of the subpubic angle is a measurement of considerable importance, as it indicates the form of the anterior aspect of the pelvis. There is sometimes a little difficulty in determining the exact angle in pelves where the inner and anterior lips of the tuberosities of the ischia project beyond the ischio-pubic rami. This, though it does not very frequently happen, occurs oftener in female than male pelves, from the internal border of the rami in the former being somewhat concave. In such cases I think it is best to exclude the ischia, and measure the angle formed by the rami only. When the internal borders of the rami are rough and are studded over with small bony prominences, as occasionally happens, it is almost impossible to measure the angle absolutely exactly. As a rule, however, when measured in the manner indicated with a suitable goniometer, it can be taken with accuracy. In the three races under consideration, this measurement shows considerable variation ; in the female pelves measured by me it averages in the Europeans  $76^{\circ}$ , in the Australians  $78^{\circ}$ , and in the Andamanese  $85^{\circ}$ .

Verneau found it to be in thirty-five European females  $74^{\circ}$ , and in two Australians of the same sex  $80^{\circ}$ . The averages obtained by Verneau and myself in the case of the European females agree very closely. Of Australian pelves, unfortunately neither of us possess a sufficient number to determine accurately what the true average size of the angle is, but in this respect the skeletons in Paris seem to correspond closely with those in the Museum of the Royal College of Surgeons.

In addition to those measurements already discussed, Professor Flower includes in his list five others, namely, the height and breadth of the obturator foramina, and the height and breadth of the acetabula, and the inter-obturator foramina breadth. As they do not appear to me to indicate specially the general

form of the pelvis, they are not included in the list of measurements necessary for that purpose, though they are useful, and indeed necessary in formulating minute differences. Of these measurements the most important is the intra-obturator breadth or the minimum width between the inner borders of the obturator foramina. The form of the obturator foramina has been frequently regarded as of great diagnostic value in determining the sex, if not also race. My observations lead me to endorse the statement of Verneau, "Il n'en est absolument rien; il est complètement faux de dire que le trou soit triangulaire chez la femme, ovulaire chez l'homme." The chief sexual difference in the form of the foramen, as I hope on a future occasion to show, is that it is larger in females than in males. The size of the acetabulum, though somewhat different in the three races considered, is proportional to the size of the pelvis of each. Its length is on an average about 1.5 mm. greater than the breadth.

The measurements just described as of primary importance are, I believe, all that are required to represent the general form of the pelvis. They number unfortunately more than I had hoped, though they are less by six than the number recommended by Professor Flower. Future observations may probably show that some of them may be replaced by more comprehensive ones, or classified amongst those of secondary importance. The essential form of the pelvis is to a great extent influenced as previously stated by the respective sizes of the antero-posterior and transverse diameters of the brim. The view of the pelvis which includes those should *prima facie* be the most important. Experience confirms this, and to show the characters of any pelvis it has been customary to represent its upper aspect. Besides showing the form of the brim, such a view shows the form and breadth of the crests and the breadth of the sacrum, and is altogether the most comprehensive that can be obtained. It is expressed by seven measurements, viz., the antero-posterior and transverse diameters of the brim, the width between the anterior-superior spines of the ilium, the width the posterior-superior spines, the maximum crest width, the breadth of the ilium, and the breadth of the sacrum. If in practice, therefore, it be found that the whole list of measurements is too numerous to give in a general description of the skeletons of any particular race or sex, it should be

remembered that most important information as to the character of the pelvis is yielded by those of its upper half, and that by giving them in osteological descriptions, anthropologists may do much to increase our knowledge of this part of the skeleton. Their value may be estimated partially by reference to the table, and by contrasting them and their indices in the three races of which measurements are given; but the fact that it is possible to draw the upper aspect of any normal pelvis from these measurements, or from their averages in any race to produce a drawing of a typical pelvis of the race, is a marked proof of their importance. Although neither the pelvis nor any other part of the skeleton is constructed upon mathematical principles, as far as my observations go, yet the relation of these measurements to one another is such as to allow of the application of mathematical methods to determine the position of the respective points between which they are taken. It is not pretended that more than a good general outline of the upper aspect of the pelvis, as seen from above when the pelvis is placed with the brim horizontal, can be thus produced; and as it is, I believe, the first attempt that has been made towards diagrammatic formulation of this portion of the skeleton from measurements only, the *modus operandi* may very probably be capable of considerable improvement, or require a few modifications. The line of antero-posterior diameter of the brim divides the pelvis into two equal halves, and is very convenient for the purpose of determining the position of the other measurements, since the various points of pelvic width lie on either side of it. To construct a diagram of a pelvis from measurements, draw a perpendicular line AB (see Pl. V.), and on it mark a point C to represent the symphysis pubis; on the line AB take a point D, situated at a distance from C, equal to the antero-posteriordiameter of the brim, and through D draw a line FF' at right angles to AB. This line I have found, after comparing several pelves, corresponds very nearly to the line of greatest crest width. Divide the line CD into three equal parts at the points M and G. The greatest transverse diameter of the brim, as a rule, is situated between the posterior and middle thirds of the antero-posterior diameter, the point G therefore corresponds to it. Through G draw a line II' at right angles to AB. On the line DA take a point N, so

that DN shall be equal to DG. After several observations, I have found that the point where crest of the ilium disappears from view behind the sacrum when the pelvis is placed with the plane of the inlet perpendicularly or horizontally, is as far behind the promontory of the sacrum as the line of maximum transverse diameter is in front of it. This, and similar observations I have been able to make accurately by means of exact tracings of pelvis taken with Broca's Stereograph. Through the point N draw a line at right angles to AB, and on it take a point L at a distance from N, equal to half the width between the posterior-superior spines of the ilia. After examining the several pelvis, I have found that the point L, though considerably higher on the crest than the posterior-superior spine, is situated in a direct line above it when the pelvis is held with the brim-line in the horizontal position. LL' will therefore correspond to the width between the spines. On the line passing through D at right angles to AB take the points EE', each distant from D equal to half the width of the sacrum, and draw through them lines parallel to AB. The greatest sacral width is usually a little behind the line of the sacral promontory at the points OO'. Likewise, take the points FF', so that FD and F'D are each equal to half the maximum crest width. The line of greatest crest width I have found to be usually situated in a line with the promontory of the sacrum, though sometimes it is somewhat further forward, and sometimes further back. On the line passing through G at right angles to AB, take the points I and I', at distances from G equal to half the width between the anterior-superior spines of the ilium, and the points H and H', at distances equal to half the transverse diameter of the brim. Through the points II draw lines parallel to AB, and on these take points K and K', so that each shall be at a distance from L and L' equal to three-fourths the iliac breadth. K and K' represent the anterior superior spines of the ilium. The line of iliac breadth, though about transverse in the erect position of the skeleton, is directed downwards and backwards when the brim is placed horizontally. After several researches, I have found that the portion of the crest seen when the brim is horizontal is usually equal to three-fourths of the entire breadth of the ilium. Lines drawn from L to K and from L to K' represent the angle at which the iliac bones are placed.



Having ascertained these different points, all that remains to be done is to draw in the outlines of a pelvis between them. This has been done in the two pelves figured in Plate V. The first is the form of the European pelvis drawn from the average measurements of fourteen European female pelves given in the general table, while the second is a typical Andamanese pelvis, drawn likewise from averages, the product of thirteen female Andamanese pelves. On comparing those two drawings with pelves of the respective races which correspond as nearly as possible to the average in all their measurements, it will be found that they represent very fairly the general form of each. The two figures show especially the characteristic difference between the races in the form of the brim and the angle at which the iliac bones are placed.

The method of producing the diagram, though it may at first sight appear somewhat complicated, is in reality quite simple, and may readily be learned. I have not yet been able to construct a similar diagram of the anterior view of the pelvis which is more difficult to represent, from the difficulty in determining the relation of the various measurements to one another. This diagram of the upper surface will, I think, prove useful in enabling us to appreciate more fully the value of measurements, since it shows that they are convertible qualities which at any time can be produced to the form of a diagram.

In Table I., I have placed the individual measurements of the transverse and antero-posterior diameters of the pelvis, from which the average of these measurements given in Table II. are taken, along with the indices obtained, first by taking the transverse diameter as the standard, and secondly by taking the antero-posterior diameter as the standard. The individual variations from these indices are indicated in the small columns between each. By giving this detailed comparison, I trust better opportunity will be afforded of deciding which of the two diameters forms the best standard. The headings of Table II. explain themselves sufficiently without further observations.

TABLE I.  
EUROPEAN ♀

No. of Skeleton.	T.D.B.	Above or below the average 138.		A.P.D.B.	Above or below the average 107.		Index A.P.D.B. × 100 T.D.B.	Above or below the average 80.		Index T.D.B. × 100 A.P.D.B.	Above or below the average 126.	
		+	-		+	-		+	-		+	-
60	143	10	...	108	1	...	76	+	4	132	6	...
159	133	...	...	115	8	...	86	...	...	116	...	10
160	143	10	...	103	...	4	72	...	8	139	13	...
161	131	...	2	115	8	...	88	...	...	114	...	12
162	130	...	3	106	...	1	82	2	...	123	...	3
163	123	...	10	103	...	4	84	4	...	119	...	7
164	146	13	...	93	...	14	64	...	16	156	80	...
165	130	...	3	115	8	...	88	8	...	113	...	13
166	137	4	...	114	7	...	76	...	4	132	6	...
167	133	...	...	103	1	...	81	1	...	123	...	3
168	129	...	4	102	...	5	79	...	1	126	...	...
170	131	...	2	102	...	5	78	...	2	128	2	...
171	117	...	16	98	...	9	84	4	...	119	...	7
172	136	3	...	110	4	...	81	1	...	124	...	2
Total	1862	40		1492	39.5		1119	34.5		1729	57	
Average	133	2.9		106.6	2.9		80	2.5		125.7	4	

AUSTRALIAN ♀

No. of Skeleton.	T.D.B.	Above or below the average 118.		A.P.D.B.	Above or below the average 109.		Index A.P.D.B. × 100 T.D.B.	Above or below the average 92.		Index T.D.B. × 100 A.P.D.B.	Above or below the average 110.	
		+	-		+	-		+	-		+	-
1043	109	...	9	127	18	...	116	24	...	86	+	24
1052	113	...	5	101	...	8	89	...	3	112	2	...
1066	125	7	...	99	...	10	79	...	13	126	16	...
1088	131	13	...	112	3	...	85	...	7	117	7	...
1398	113	...	5	104	...	5	92	...	...	108	...	2
Total	591	19.5		543	22		461	23.5		549	25.5	
Average	118.2	3.9		108.6	4.4		92.2	4.7		109.8	5.1	

TABLE I.—*continued.*

ANDAMANESE ♀

No. of Skeleton.	T.D.B.	Above or below the average 103.		A.P.D.B.	Above or below the average 99.		Index A.P.D.B. × 100 T.D.B.	Above or below the average 96.		Index T.D.B. × 100 A.P.D.B.	Above or below the average 104.	
		+	-		+	-		+	-		+	-
1003	102	...	1	91	...	8	89	...	7	103	...	1
1012	110	7	...	109	10	...	99	3	...	101	...	3
1013	102	...	1	100	1	...	98	2	...	102	...	2
1014	197	...	6	96	...	3	99	3	...	101	...	3
1016	100	...	3	92	...	7	92	...	4	109	5	...
1018	106	3	...	97	...	2	91	...	5	109	5	...
Oxf.	102	...	1	110	11	...	108	12	...	93	...	11
Thompson A.	104	1	...	98	...	1	94	...	2	106	2	...
Thompson B.	103	...	...	89	...	10	86	...	10	116	12	...
1477	104	1	...	105	6	...	101	5	...	99	...	5
1478	102	...	1	102	3	...	100	4	...	100	...	4
1479	110	7	...	117	18	...	103	10	...	94	...	10
1480	95	...	8	83	...	16	87	...	9	114	10	...
Total	1337	20		1289	48		1250	38		1347	36.5	
Average	102.8	1.5		99.1	3.8		96.1	3		103.6	2.9	

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TABLE II.—AVERAGE MEASUREMENTS AND INDICES OF THE PELVES OF DIFFERENT RACES.

	S.L.	S.B.	A.S.S.W.	C.W.	P.H.	I.B.	P.S.S.W.	A.S.W.	P.I.D.	A.P.D.B.	T.D.B.	A.P.D.O.	T.D.O.	S.P.A.
14 European, ♀	101	118.3	231.5	271	201.7	157.2	84.2	117.3	91.4	106.6	133	116	115.9	76°
5 Australian, ♀	91.4	104.6	198.4	240.6	184.4	142	85	106	82.2	108.6	118.2	107.6	104.8	78°
18 Andamanese, ♀	91.4	97	172.1	207.7	167	122.7	79	95.8	76.4	99.1	102.8	100.6	93.1	85°
<i>Indices of the above Measurements.</i>														
14 Europeans	75.3	88.7	173.8	203.8	151.8	118	63.1	88	68.4	80	100	87.2	87.2	...
5 Australian	77.1	89	167.8	204.1	155.9	120.3	72	89.8	69.5	92.4	100	91.5	89	...
18 Andamanese	88.3	94.2	167	201.9	162.1	119.4	76.7	93.2	73.8	96.2	100	93	90.2	...
<i>Averages of similar or nearly similar Measurements given by Verneau.</i>														
85 European, ♀	101	116	222	266	197	156	74	...	93	106	135	111	...	74°
2 Australian	101	107	182	237	176	138	79	...	91	100	125	115	...	80°
<i>The Indices of the Measurements given by Verneau.</i>														
35 Europeans	74.8	85.9	164.4	197.1	...	115.5	...	...	68.9	78.5	100	82.2	...	...
2 Australians	80.8	85.6	145.8	189.6	...	110.4	...	...	72.8	80	100	92	...	...
<i>The combined Averages of the preceding Measurements.</i>														
49 Europeans	101	116.6	not comparable	267.3	198.1	156.3	not comparable	...	not comparable	106.2	134.8	112.5	not comparable	74.5°
7 Australians	94.1	105.8	...	239.7	182	140.8	...	...	...	106.1	120.1	109.7	...	78.6°
<i>Indices of the combined Measurements.</i>														
49 Europeans	74.8	86.6	...	197.7	146.6	115.8	...	...	...	79	100	88.7	...	...
7 Australian	78.3	87.5	...	200	151.6	117.2	...	...	...	88.3	100	91.3	...	...

## THE CRANIAL CHARACTERS OF THE ADMIRALTY ISLANDERS.<sup>1</sup> By Professor WM. TURNER, M.B., F.R.S.

(Abstract.)

THE skulls described in this communication were obtained in March, 1875 by the Scientific Staff of H.M.S. Challenger, at Wild Island, one of the Admiralty group. Their description has been entrusted to me by Sir C. Wyville Thomson, and it is by his kind permission that I am enabled to bring them before the notice of the Anatomical Section of this Congress. The collection consisted of eleven skulls, a calvaria, and a face; the greater number were smeared with a red pigment, and one had an artificial nose and eyes modelled in a black material. Several had holes broken through the squamous-temporals, in which sticks had been inserted for suspending the skulls. The crania were all elongated antero-posteriorly, not flattened or ridge-shaped at the vertex, nor with strong supraciliary ridges nor glabella. No artificial parieto-occipital flattening. The crania were all adult, and probably consisted of seven males and five females. Wormian or other triquetral bones were not uncommon. In two specimens the alisphenoid and parietal articulated in the region of the left pterion, and in one specimen the right parietal was completely separated into an upper and a lower division by an antero-posterior denticulated suture. One specimen had a third occipital condyle, and in the same skull the nasal bones had not been developed, and the nasal spine of the frontal and the ascending processes of the superior maxillæ formed the bridge of the nose.

The mean cephalic index was 70, that of the males 71, of the females 69. The mean vertical index was 72, that of the males 73, of the females 70. The approximate cranial capacity was 1310 cubic cent., that of the males 1422, of the females 1170. The mean facial index was 65, that of the males 62, of the females 68. The mean gnathic index was 103, which was also the average in the two sexes. The mean nasal index was 48, that of the males 51, of the females 44. The mean orbital index was 86, which was also the average in the two sexes. The mean palato-maxillary index was 112, which was almost the same in the two sexes. The crania therefore were markedly dolichocephalic, somewhat higher than broad; microcephalic for the females, but mesocephalic for the males; as a rule cryptozygous: on the line between mesognathism, and prognathism; on the line between the leptorhine and meso-rhine as regards the nasal index; meso-sene as regards the orbital index; with no disproportion between the length and breadth of the palato-maxillary arch.

The paper concluded with a comparison of these crania with those of the Australian, Melanesian, and proper Polynesian races. Though

<sup>1</sup> Read before the International Medical Congress in London, August 4, 1881.

dolicocephalic like the Australians, they were not so massive, nor with so strong a glabella and supraciliary ridges; nor roof-shaped like the Australians; neither were they like them platyrrhine, nor so distinctly prognathic. They differed materially from the brachycephalic or sub-brachycephalic Polynesians, like the Samoan Islanders. They were not so dolicocephalic as the natives of the mountainous region of Viti Levu, one of the Fijian islands, described by Professor Flower, who are probably an unmixed Melanesian race, and in whom the cranial index was as low as 66. The Fijian mountaineers were also strongly phenozygous and platyrrhine. The Admiralty Islander crania much more nearly resembled those of the dolicocephalic people inhabiting the coast-line of New Guinea. Like New Guinea, these islands were originally colonised by the great Melanesian race, but from time to time have doubtless been visited, and it may have been partially occupied by other races, and in this manner modifications in their original race characters may have arisen.

## Report on Physiology.<sup>1</sup>

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SELECTED ABSTRACTS by WILLIAM STIRLING, M.D., Sc.D.,  
*Professor of the Institutes of Medicine (Physiology) in the  
University of Aberdeen.*

### ON THE FUNCTIONS OF THE CORTEX CEREBRI.

H. Munk (*Über die Functionen der Grosshirnrinde*, Berlin, 1881) has published in a collected form several contributions which he has made to various journals on the above subject (1877-80).

In the introduction a short *resumé* is given of the experimental researches on the brain from the time of Flourens to Fritsch and Hitzig.

I. EXPERIMENTS ON DOGS.—If a line be drawn vertically from the end of the Sylvian fissure towards the falx, it divides two well-marked areas of the cortex from each other. In the extirpation experiments dogs of middle size were used, and circular portions—15 mm. in diameter and 2 mm. in depth—of the cortex were removed from the convexity of the frontal, occipital, and temporal lobes. Extirpations practised in front of the imaginary line always caused disturbances of motion, whilst similar operations practised behind this line never caused—not even a trace—of motor disturbance. The posterior section of the convexity of the brain contains two functionally distinct areas; the one area includes the occipital lobes, and has certain relations to the sense of sight; the other is formed by the cortex of the temporal lobes, and is related to hearing. The *anterior* section of the cortex comprises the area for the sense of touch (in the widest sense of the term) for the body. The surface of the cortex cerebri may therefore be divided into three areas. The spheres or areas for (1) touch, (2) sight, and (3) hearing.

1. The *Sensory Area* (Fühlsphäre).—Former investigators, depending on the results of stimulation of the brain and the results of removal of certain parts thereof, were almost all of opinion that motor or psychomotor centres were situated in this area. These “motor” centres involve only a limited area of the cortex around the sulcus cruciatus, while the functions of the remaining portions were unknown. Munk is of opinion (1) that the motor phenomena do not necessitate the assumption that these centres are “motor,” but only that these regions are the seat of the perceptions which result from the sensory touch

<sup>1</sup> To assist in rendering this report more complete, authors are requested to send copies of their papers to Dr Stirling, University, Aberdeen.

impressions coming from the body, and accordingly the motor perceptions in the sensory spheres (Fühlsphäre) are the cause of the so-called voluntary movements; (2) that the sensory area is not limited to the cortex of the temporal lobe, but also embraces the cortex of the frontal lobe. The sensory area of each half of the body consists of a series of regions, each of which has a special relation to special parts of one half of the body, so that injury of one region always causes disturbance of the sensibility and sensory perceptions only in the corresponding part of the body. Munk has isolated seven regions within the sensory area which are locally separate from each other, and when they are destroyed singly produce characteristic phenomena. From behind forwards they are a region for (1) eye, (2) ear, (3) hind limb, (4) fore limb, (5) head, (6) neck, (7) trunk.

With regard to the results which follow from experimental interference with these regions we refer to the original. The sensory area is the place where the perception of the impressions from the sense of touch takes place, and the perception or conception and the memory pictures of impressions have their seat. The perceptions which arise from the sense of touch are various—(a) conscious skin impressions which lead to sensations of *touch* and *pressure*, and which form the basis of our conceptions of touch and pressure; (b) impressions from muscles—muscle sense—which, with the impressions proceeding from the skin, give us a notion of the position of parts of our bodies in space as in passive movements—*perceptions of position*; (c) impressions from active muscles, the *nerve perceptions*. These, when united with pressure and muscle sense, give on the one hand perceptions of movement, and on the other perceptions of touch, perceptions of the form, extent, &c., of objects which the active parts of the body come in contact with or touch.

Suppose a large part of the cortex—within the region of the fore limb—to be removed from an adult animal, it is obvious that the disturbances of “sensibility and motion” may be classified according to the above groups. Gradually the sensory perceptions are formed again anew, and first the simpler perceptions (pressure and position) and then the more complex perceptions follow. Extirpation of small portions produces only partial loss (at first of perception of motion and touch, then of position and pressure). Extirpation of large areas complete loss of sensory impressions, proceeding from the body (paralysis of sensation, motion, and sensibility of that part of the body). If the extirpation be very large, only pressure and position may be restored. The complete destruction of the sensory area must result in permanent loss of all feeling and perceptions of impressions from the parts of the body.

2. *The Area for Vision* (Sehsphäre).—If a circular portion of the cortex cerebri—15mm. in diameter and 2 mm. in depth—be removed from the convexity of the occipital lobe (middle sized dog), near its posterior and upper end, after three to five days, when the inflammatory disturbance has subsided, the smell, taste, motion, sensation, &c., of the animal will be normal, but it exhibits peculiarities of vision. The dog moves, without, however, coming in contact with objects; it avoids obstacles placed in its path. It no longer knows, however, indi-



viduals, nor the objects which belonged formerly to its daily observation and experience—neither food nor conditions calculated to inspire fear—as fire, the whip, nor even the attendant once his friend. The dog has become mentally blind (“Seelenblind,”) i.e., it has lost the visual conceptions, and the memory pictures of its former visual perceptions which it possessed, so that it neither knows nor perceives what it sees! but the dog sees! the visual impressions reach his consciousness, become perceptions, and cause conceptions of the form and position of external objects, so that new visual conceptions and memory pictures of visual impressions are acquired. The dog learns to see anew, just as in its youngest condition, and after three to five weeks the sense of sight is completely restored.

These experiments necessarily lead to the conclusion that the visual area is of much greater extent than that involved in the operation, and this is confirmed by experiment. It embraces the whole occipital lobe, with the side of the gyrus medialis directed towards the falx. Within this region the central elements, in which the fibres of the optic nerve end and the visual perceptions take place, are regularly and continuously arranged, so that neighbouring retinal elements always correspond to neighbouring perceptive elements of the cortex. Each retina is arranged with its most external lateral part, with the most external lateral portion of the visual area of the same side, whilst the much larger and remaining portion of each retina belongs to the larger remaining visual area of the opposite side, so that one can imagine the retina projected on to the visual area, so that the lateral margin of the retina corresponds to the lateral margin of the visual area, the inner margin of the retina to the median margin, the upper margin of the retina to the anterior margin, and lastly, the lower margin of the retina to the posterior margin of the visual area.

The most acute point of vision in the dog is in the outer half of the retina, and belongs to the position  $A_1$  of Munk of the visual area. For this reason, in spite of the large visual area, the memory pictures of visual perceptions are collected in  $A_1$ . The same spot of the retina is always used for distinct vision, hence the distinct perception of objects falls upon the position  $A_1$  of the visual area. With regard to the course of the fibres from the retina to the cortex, a semi-decussation takes place in the dog; the non-decussating bundle of the optic nerve runs from the most external lateral portion of the retina to the most external lateral visual area. Within the decussating bundle all the fibres become displaced, so that the fibres which are arranged from right to left come to lie from left to right.

If both visual areas are completely removed, of course the optic fibres are stimulated by light as before, and this causes reflex effects—movements of the iris—through central parts situated below the cortex, but light is no longer perceived, and perceptions of light no longer occur. Complete blindness of the cortex occurs for all time.

The *mental-blindness* is only “blindness of the cortex” (*Rindenblindheit*), in which the end elements of the point of direct vision

in the cortex of the cerebrum are destroyed ( $A_1$ ). Still the mental-blindness has this peculiarity, that it includes in itself the *definite* outcome of all the "conception elements," in which the memory pictures of former visual impressions were contained, as it were, in a latent condition. The "conception elements" are contained in those parts of the visual area which are united with the points of distinct vision, and its surroundings in the retina. This mental-blindness may be gradually restored, for the dog directs its attention to the visual impressions which reach it through other portions of the retina. Thus in time there is recovery from the mental-blindness, whilst there is only partial recovery from the "blindness of the cortex."

3. *The Auditory Area* lies in the temporal lobe. Extirpation of both auditory areas in their entire extent causes complete "cortical deafness" (Rindentaubheit). These experiments are very difficult, and often fail, on account of the difficulty of the operative procedure. There is a spot ( $B_1$ ) in the auditory area on which the auditory conceptions of the dog depend. It corresponds to the spot  $A_1$  in  $A$ . Extirpation of  $B_1$  causes "mental deafness" (Seelentaubheit). Auditory conceptions are lost; but, as with the visual areas, there may be restitution in other parts of the auditory area within a few weeks.

II. EXPERIMENTS ON APES.—The *sensory areas* correspond in position and extent pretty closely with those of the dog. The regions for the eyes are of great importance. It includes the gyrus angularis, exactly that part which Ferrier calls the visual centre, in the narrower sense of the word. Extirpation of this region, which goes too far back, causes disturbances of visual impressions and visual perceptions; if too far forward, there is, in addition, interference with touch and the perceptions of sensory impressions from the head and extremities. After extirpation of the eye region on one side, *e.g.*, left, the following are the results:—If the bulb or palpebral conjunctiva of the right eye is touched with a needle, there is slight winking; but one can press and prick the eyeball, and the animal remains quite quiet, whilst the slightest touch of the left eye causes characteristic painful sensations and protective movements. If the finger is brought close to the left eye, this act is always followed by winking; but if the same is done to the right eye, winking does not occur until there is direct contact with the eye-lashes or eye-lid. It would seem, therefore, that the cortex cerebri can no longer set the sphincter palpebrarum in action. Slight ptosis and well-marked lachrymation are sometimes observed, and the movements of the eyeballs are somewhat impaired; but changes in the pupil were never observed as a sequel of the experiment. On account of the complexity of the motor acts the impairment and the loss of the perceptions of space, touch, and motion were well marked.

*The Visual Centres.*—The cortex of the well-defined occipital lobes are the anatomical seat of these centres. Unilateral total extirpation of the cortex on the convex surface causes the monkey to become permanently hemiopic blind for the halves of both retinæ on the side opposite to the injury; but usually there is a trace of visual perception left from the difficulty of removing the under surface of the occipital

lobes on both sides. It seems that each macula lutea has relations with the visual centres in both hemispheres.

In conclusion, the author communicates his experiments on the *olfactory* centre of the dog; and from pathological changes he comes to the conclusion that the gyrus hippocampi is the seat of this centre. (*Centralblatt*, 1881.)

#### ON THE MOVEMENTS PRODUCED BY STIMULATION OF THE BRAIN.

François-Franck and Pitres (*Travaux du Laboratoire de M. Marey*, IV., 1878-79, p. 418), applied electrical stimuli to the grey matter, and also to the underlying white substance. A single induction shock produced a simple muscular contraction, just like that produced by stimulation of a motor nerve. When a series of slow stimuli were applied the corresponding muscles replied with single contractions, and when the stimuli were rapid, with tetanus. The same number of stimuli is required to produce tetanus whether the brain, nerve, or muscle is stimulated. If a series of "inadequate" stimuli are used instead of one "effective" stimulus, then "summation" of the weak stimuli occurs. The contraction occurs some time after the application of the stimulus; there is a latent period, which is longer the larger the animal. The strength of the stimulus makes no difference, but the distance of the centre from the muscle does, on the latent period. The authors conclude that the nerve impulse proceeding from the brain travels at the rate of 10 metres per second.

The authors then discuss the associated movements which occur after stimulation of the centre of one side. These movements are neither similar nor synchronous; the muscles of the opposite side contract sooner ('05") and stronger, those on the same side after '065". The associated movements do not take place if the nerves on that side are divided before the experiment. They think that the impulses cross in the pons and medulla, so that the movements may be reflex.

In animals which are not narcotised, epileptic convulsions occur with extreme ease, and the attack may be local, hemiplegic, or general. The attack is generally local, and when the right side is stimulated certain parts are convulsed before others. There are two periods in each attack—one tetanic, the other clonic. It begins with tetanus, which lasts a short time and is not in proportion to the total duration of the attack. In the second period there are short sharp contractions of the muscles, which become stronger and longer. The attack may last from a few seconds to fifteen or twenty minutes; the attacks may follow each other rapidly and kill the animal.

With regard to the *white substance*, a bundle of fibres springs from each centre, passes through the centrum ovale, enters the inner capsule, and retains its functional independence throughout its entire intra-cerebral course. The fibres are excitable, but not in an equal degree, throughout their entire course, the excitability being less near the convolutions than in the grey matter, whilst it is much greater in the inner capsule. Single shocks cause simple contractions, rapid

shocks tetanus. The contractions occur more rapidly, so that it is obvious the grey matter offers more resistance to the impulses. Epileptic attacks are not produced. Stimulation of the inner capsule produces violent tetanus, but with dissociated movements. The great excitability is due to the grey nuclei of the corpus striatum, although these nuclei themselves are not excitable (*Centralblatt*, No. 13, 1881).

#### PHYSIOLOGY OF THE RABBIT'S BRAIN AND ITS NERVES.

A. Christiani (*Monatsb. d. k. Akad. d. Wissensch. zu Berlin*, February 1881, and *Centralblatt*, No. 25, 1881), finds that—

1. Stimulation of the end of the optic nerve exposed in the orbit of a rabbit, and the branch of the trigeminus which lies close beside it, influenced the respiration, and changed the heart-beat even to stopping it—even when weak currents were used, so that there was no chance of secondary circuits being formed. Stimulation of the optic nerve accelerated the respiration in the inspiratory phase, and that of the trigeminus in the expiratory phase. After removal of the cerebrum and the corpus striatum without ligature of the carotids, the intracranial stimulation of the optic gave the same result. Light had the same effect, only it acted more powerfully after removal of the cerebrum. Stimulation of the acoustic nerve by means of its "adequate" stimulus caused inspiration, and was also more effective after removal of the cerebrum. A limited area was found in the interior of the optic thalamus, a little above the floor of the third ventricle, where stimulation caused a standstill of the diaphragm in inspiration, or caused deepened inspiration and accelerated respirations, this the author calls the "inspiratory centre of the third ventricle," and it seems to be associated with the pupil-dilating centre. Strychnine excites both centres, whilst during chloral narcosis the excitability of the inspiratory centre is greatly diminished and the pupil contracted. Section of the vagi does not affect the relations of the inspiratory centre. This centre can be removed with a trocar when an expiratory centre in the corpora quadrigemina close under and near the aqueduct of Sylvius is found, whose stimulation caused, after section of both vagi, active expiratory efforts, or stand still in ordinary or active expiration. After cessation of the stimulation there is a compensatory accelerated inspiration, and often a peculiar cry. This centre, which is scarcely affected by chloral, is associated with the centre for contraction of the pupil. The inspiratory centre is active even during apnoea. Removal of the cerebellum scarcely affects the respiration, nor does a section behind the corpora quadrigemina without a deep lesion of the pons. After this the reflex effect on the respiration from the tactile (inspiratory) and painful (expiratory) fibres of the trunk was increased. Deep injuries to the pons cause death after a few respirations. The continuation of these centres lies in the pons and medulla. Electrical stimulation of the vagi in the neck always causes inspiration. Even after removal of the cerebrum and corpora

striata inspiratory effects are produced through eye and ear, sensory nerves of skin, and certain fibres of vagus; whilst the other fibres of the vagus are inhibitory and expiratory, and so are those of the trigeminus and the pathetic fibres of other sensory nerves.

2. If both cerebral hemispheres and the corpora striata are removed with a minimum loss of blood, although the carotids are not ligatured, when not held, the rabbit appears like an animal with its brain, only it falls more readily into sleep (!). When it awakens spontaneously it avoids obstacles, &c. This condition is changed at once when a section is made in front of the corpora quadrigemina without injury of the pons, i.e., near the inspiratory centre of the third ventricle. The animal lies on its side; the co-ordination for standing and locomotion is abolished. If the optic thalami and anterior layers of the corpora quadrigemina are separated it is scarcely possible to discharge a reflex movement by mechanically stimulating the extremities or trunk, but it is easy to do so from the sensory nerves of the head and neck, and from the trigeminus. If the section is made behind the corpora quadrigemina the rabbit falls in opisthotonos; the reflex excitability of the head and neck is greatly increased.

#### ON THE EFFECTS OF ANÆSTHETICS ON DIFFERENT REFLEX PHENOMENA.

Professor Eulenberg of Greifswald finds that during the narcosis produced by the inhalation of various anæsthetics (rabbits, dogs), the reflex excitability of the skin and tendons and other surfaces varies.

1. Certain anæsthetics (*chloroform*) cause at first a rapid temporary increase of individual reflexes (patellar reflex), then diminution and disappearance of the reflex. The patellar reflex always disappears sooner than the corneal reflex; while on cessation of narcosis the corneal reflex always returns before the patellar. In man the nasal reflex remains longer intact than the corneal, which agrees with the observations of O. Rosenbach on sleeping children. The nasal reflex disappears only in deep hypnosis.

2. *Ether*, and to a less degree æthyl and æthylene compounds, often cause an enormous increase in the reflex excitability (tendon, periosteal, patellar, tibial, and foot reflexes in rabbits), and this condition may last much longer than the narcosis. The corneal reflex is abolished relatively late in ether narcosis, indeed it is seldom completely abolished.

3. The doubtful chlorine compounds, viz., *æthylene chloride*, *æthylidene chloride*, *methylene chloride*, when inhaled by rabbits and dogs, cause diminution (without any previous increase) of the reflex; the corneal reflex always disappears earlier than the patellar, whilst the latter always returns before the former.

4. *Bromæthyl* acts but slowly, if at all, at a late period on reflex action; the patellar reflex is gradually diminished without any previous increase; the corneal becomes weak, but is seldom quite abolished.

It is obvious that the reflex apparatus stands in no direct relationship to the time when the anæsthetic affects the psycho-motor and sensory centres. The depth of the narcosis, the extent and degree of the anæsthesia are not proportional to the condition of single reflexes or groups of reflexes.

Hypnotics and sedatives have a similar action.

*Morphia* (.5 grms. subcutaneous into a rabbit) affects the reflex apparatus slightly. *Chloral hydrate* (1.25-1.5 grms. rabbit, subcutaneously) on the whole acts like chloroform, but without initial increase of excitability, the patellar reflex disappears gradually in advanced hypnosis, whilst the corneal in non-lethal doses, although weakened, is often present. *Bromide of potassium* (1-2 grms. rabbit) at first increases the patellar reflex and then diminishes it. Similar actions are possessed by *chloral bromide* and *quinine bromide*. In death by *asphyxia* the corneal reflex usually disappears before the patellar, and the latter usually just before the exophthalmos, dilatation of the pupil and the last dyspnoic convulsions. (*Centralblatt*, No. 6, 1881.)

#### THE INFLUENCE OF NERVES ON THE NUTRITION OF THE VESSELS AND THE PRODUCTION OF ANEURISM.

S. Lewaschow (*Petersburg Med. Wochensch.* No. 31, 1880), proceeded on the assumption of Botkin, as the result of clinical experience, that aneurisms arise from nervous influences. A ligature moistened with dilute sulphuric acid was drawn through the sciatic nerve (cats and dogs) just where it emerges from the pelvis; the nerve was replaced and the wound sewed up. This operation was repeated at short intervals, as long as the animal lived, on parts of the nerve situated immediately beyond, i.e., peripherally to the seat of former experiment. In some animals little disturbance took place, in others epileptic convulsions came on. There was atrophy of the thigh, leg, and foot of the extremity experimented on; the nerve was thickened, red, and united to the surrounding tissues (this occurs very frequently when a nerve is divided). Swellings of moderate size were found in the interior of the vessels, more especially where branches were given off.

#### ON THE EFFECT OF STIMULATION OF SENSORY NERVES ON THE FUNCTION AND NUTRITION OF THE HEART.

N. P. Simanowsky (*Petersburg Med. Wochen.* No. 52, 1880, *Centralblatt*, No. 11, 1881), proceeded from the well-known fact that a biliary calculus impacted in the bile-duct produces amongst other symptoms a change in the action of the heart. A biliary fistula was made on a dog, and the gall bladder was stimulated by an induced current from a Du Bois' induction machine. All loss of bile was avoided, and fine electrodes provided with ball points were introduced into the gall bladder. Mere contact of the electrodes with the mucous membrane of the gall bladder produced violent pain, and when the electrodes

were moved within the gall bladder a resistance was experienced, very probably due to contraction of its muscular coat. Weak stimulation usually increased the number and force of the heart's contractions, whilst strong currents diminished the frequency and in addition produced arrhythmia, whilst the energy at one time increased, at another diminished; during and for an hour after the stimulation, the tension in the right femoral artery seemed to be greater than in the left. The frequency of the respirations and the rectal temperature were increased by strong stimulation. Several minutes after the stimulation paraplegia was observed. The blood-pressure, as measured in the femoral artery, was in direct relation with the strength of the stimulus. A decided slowing of the heart's action accompanied the increase of blood-pressure.

When the terminations of the vagus in the heart were paralysed with atropine, stimulation of the gall bladder increased the blood-pressure, but there was no slowing of the heart's action, while the increased pressure did not occur after section of the vagi.

Stimulation of the pelvis of the kidney with an electrical current caused increase of the blood-pressure, whilst touching or compression of the kidney caused increase of blood-pressure with slowing of the heart-beat.

#### ON APNŒA.

J. Gad (*Habilitationsvorlesungen*, Wurzburg, 1880) refers especially to two of the phenomena of apnœa. After interruption of the artificial respiration the blood remains bright scarlet in colour for some time, and the arterial blood, which in the later stage of the apnœa does not contain more O than arterial blood, does not excite respiration as under normal circumstances. With regard to the first point one can convince himself that the blood in the left heart of a rabbit becomes venous within a few pulse-beats after the respiration is arrested by opening the thorax and inspecting the heart, whilst after copious artificial ventilation it remains bright-coloured for a longer time. But in the latter case of continued apnœa the blood from the beginning returns to the right heart in a venous condition, the cause of the continued arterial coloration is not due to hyper-oxygenation of the blood, but is due to the fact that the air of the lungs from the artificial ventilation is better fitted to arterialise the venous blood returning to the lungs. That the arterial blood, which, although bright red, contains less oxygen than would cause dyspnœa under ordinary circumstances, begins at last to cause respiratory movements, is due to the mechanical stimulation, *i.e.*, stretching of the terminations of the vagus in the lungs, which, acting as a cumulative after effect, produces a diminution of the excitability of the inspiratory centre. This is shown by the following experiment: If, after copious artificial ventilation of the lungs, the conduction of both vagi is suddenly arrested by cooling (under 0°) the apnœa is of shorter duration than in similar experiments with intact vagi (*Centralblatt*, No. 5, 1881).

## ON CHEYNE-STOKES' PHENOMENON.

(O. Sokolow and B. Luchsinger (*Pflüger's Archiv*. xxiii. 283) find, when frogs are immersed in water in a closed vessel until complete paralysis of the central nervous system occurs, on being placed aside, when they begin to recover they exhibit a periodicity in their respiratory movements resembling the Cheyne-Stokes' phenomenon—groups of respirations separated from each other by long pauses. In each group the intensity of the single respirations increases at first and continues for a time, and then falls pretty rapidly, or the descending part of the attack may be absent. As recovery proceeded the number of respirations in a group increased, and the pauses diminished. Cold blood animals are specially suited for these experiments in consequence of their tolerance of disturbances of the circulation. It is possible to separate changes due to alterations in the excitability of the medulla oblongata from those due to alterations in the blood. All that is required is to interrupt the circulation for a sufficient time. In this way it is possible to decide whether this phenomenon is due to periodic alterations of the excitability of the respiratory centre (Traube) or periodic changes in the stimulus (Filehne). The easiest way to produce temporary paralysis of the nervous system is occasionally to ligature the aorta. The experiments proved the untenability of Filehne's theory, for (a) during the apparent death, notwithstanding paralysis of the chief vaso-motor centres (section of spinal cord above the brachial plexus), the periodicity of respiration continued; (b) there was no change in the blood-pressure during the Cheyne-Stokes' phenomenon; (c) the phenomenon occurred also during asphyxia; (d) blood and circulation of the blood might be absent (salt frog), and the periodicity of the respiration continue. The phenomenon does not depend on the integrity of the vagi. They ascribe the phenomenon to the respiratory centre itself, and they regard "the increased stimuli and the diminished potential energy of the tissues" as the fundamental conditions for its production. The phenomenon is furthered in mammals by deep narcosis (morphium, ether, chloral), for, as the energy of the respiratory centre diminishes, a dyspnoic condition of the blood and tissues is developed. In cold-blooded animals the dyspnoic stimulus is wanting, but it can be supplied by stimulating drugs (strychnin, picrotoxin).

It is to be remarked that other organs which act rhythmically yield similar "groups"—lymph hearts of frog (Sokolow and Luchsinger); heart of frog (Luciani, Rossbach); arrhythmical contractions of the heart after greatly increased intracardiac pressure (Heidenhain); rhythmical contractions of medusa (Steiner).

The pauses are explained by a greater exhaustion of the organs—the formation of groups with the increase and diminution of the respirations from the doctrine of the "summation of stimuli." The excitability of a nervous organ increases when it is excited to action after a period of rest, but falls when it continues active for a long time (Sanders-Ezn, Stirling, Kronecker, and Markwald for the respiratory centre). Ligature of the aorta, which causes periodic respirations,



causes a long-continued increasing stimulus and dyspnoea, and simultaneously an increasing loss of potential energy. The respiratory centre is exhausted by one respiration, then follows a long pause. During this period the potential energy rises, and therewith an increased excitability, and when it reaches a certain amount there is a discharge—after a long pause—a respiration. The excitability of the centre, however, is thereby increased, and the next respiration made more easy, the following respirations increase in intensity until the potential energy of the organ is at last exhausted; the single respirations decrease in force and frequency, and thus a period of rest occurs suddenly, which permits of a further accumulation of energy. The periodicity necessitates the assumption of a diminution of the nutrition, especially of the supply of blood containing O. With good nutrition, where in the unit of time as much potential energy is transformed as is supplied, the rhythm depends on it. The authors recognise in the periodic increase and diminution of the excitability through action a biological law of the most general application (*Centralblatt*, No. 9, 1881).

#### ON THE DIGESTION OF FAT.

Th. Cash (*Arch. f. Anat. u. Physiol. Physiol. Abth.* 323, 1880) has made under Ludwig's direction some most important experiments on the relative part taken by the stomach and pancreas in the digestion of fat. Ever since the time of Bernard a very high emulsifying power has been ascribed to the pancreas, while at most the stomach is said to digest the envelopes of the fat-cells, liberating the fat, which is further acted on by the bile and pancreatic juice.

Cash kept a dog eighteen hours without food, then fed it on a meal of fat and starch, and killed it four hours afterwards; the small intestine was ligatured at intervals of a foot; these compartments were opened, their reaction tested, and their contents placed in beakers for analysis. The reaction of the same intestine from pylorus to large intestine was acid (contrary to the usual acceptance); the fat was in the form of drops or globules; an emulsion was never observed. As the lymphatics contained fat in the form of an emulsion, it is obvious fat can be absorbed in a non-emulsionised form.

The next question investigated was whether decomposition of fat takes place in the small intestine. Perfectly neutral fat was used. In the *stomach* neutral fat, fatty acids, and an acid, perhaps lactic, were found. In the *small intestine* neutral fat, fatty acids, a little bile acids, and perhaps lactic acid; the *chyle*—neutral fat and fatty acids. Even in a dog, whose pancreatic ducts were ligatured several days before, the thoracic duct contained an emulsion after feeding with fat. It seems certain, therefore, that fats may be split up in the stomach, and, in fact, artificial digestion of fat with the mucous membrane of the stomach, and dilute HCl, yielded a small quantity of fatty acids.

#### EXCRETION OF UREA.

H. Oppenheim (*Pflüger's Archiv.* xxiii. 46) has investigated the

amount of urea excreted under various physiological and pathological conditions.

1. *Normal Condition.*—On a diet of 400 grms. bread, 300 grms. flesh, and 950 grms. milk, the excretion of urea within four days became nearly constant, the variations being on an average about 1 gm. The mean of seven days was 34.6 grms. = 16.2 N. In the fæces 1.1 gm. N was given off, giving 17.3 grms. against 18.9 grms. taken in with the food.

2. *Distribution of Urea during the day; the amount of Urea excreted depends on the time of taking albuminous food.*—In twenty-four hours 34.80 grms. were excreted, i.e., 1.45 grms. per hour. In the first four hours after dinner (mid-day) .24 grms. per hour above this average was excreted; in the following four hours, during which food poor in albumin was taken, the increase reached .54 grms. per hour; during night the hourly excretion fell below the mean, and also in the morning.

3. *Commencing Hunger.*—During a day of fasting 23.91 grms. were excreted, i.e., 10–11 per cent. less. The weight fell 1 kilo.

4. *Large quantity of Water.*—Four litres of water in twenty-four hours increased the water 3000 cc., and the urea 5 grms.; the 2 litres taken after dinner increased the urea within the first four hours to 6 grms.; later there was a diminution.

5. *Coffee.*—When a decoction of 41 grms. coffee was taken the urea was 31.97 grms.; and when no coffee was taken on two days, 33.21 and 33.60 grms. The difference, however, was compensated by a higher proportion of N in the fæces.

6. *Quinine.*—With 2 grms. quinine the urea was increased 4 grms.

7. *Sweating.*—Injection of .02 grms. of pilocarpin caused sweating, and the observer had to drink 500 cc. more than usual. There was no change either in the total quantity or periods of urea excretion.

8. *Muscular Exertion.*—The usual view is that during muscular exercise non-nitrogenous substances are used up, and the nitrogenous are only used when there is a deficiency of non-nitrogenous substances in the body (Kellner), so that usually muscular exercise does not cause an increase of urea. The author is of opinion that an important factor has hitherto been omitted, viz., the dyspnoea during muscular exercise. A. Fränkel's experiments showed that diminished supply of O caused an increased decomposition of albumin, and it is possible that in certain cases the action of dyspnoea aids this, and the author finds that when muscular exercise is taken so as to develop dyspnoea (climbing a hill) the amount of urea is increased 3–5 grms.—(From Salkowski's Report).

#### EXPERIMENTAL RESEARCHES ON POLYURIA.

R. Moutard-Martin and C. Richet (*Arch. de physiol. norm. i.*, 1881; *Centralblatt*, 1881) investigated the action of several substances on the secretion of urine in dogs when injected into the blood. The abdomen was opened, the ureters exposed, a T cannula was introduced into them, and the amount of urine secreted noted.

*Injection of luke-warm water* in very small amount had no effect; 5-20 grms. per kilo. weight of animal slowed, and 30 grms. and more stopped the secretion. The slowing effect was more noticeable when the secretion was previously increased by other substances. The secretion in the intestine was not increased, but was markedly so by the injection of sugar. The urine after water-injection was bloody.

*Sugar* (cane—invert—grape—milk, to 1 grm. or more per kilo. of dog) produced great polyuria with glycosuria, but there was no blood or albumin in the urine. Relatively the amount of urea was less, but absolutely greater. Dextrin increases the urine. The bile and succus entericus were also greatly increased by sugar and dextrin, and both substances were found in the intestine.

Glycerine (in small amount), urea, urine, phosphate of soda, ferrocyanide of potassium, iodide of potassium, chloride of sodium (4-8 centigrammes per kilo), also increase the urine, but NaCl causes bloody urine. The increased secretion began when the substance injected began to be excreted.

Gum diminished or abolished secretion, and the blood-pressure became very high, while the above substances acted with a normal or abnormally low pressure.

## Anatomical Notices.

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NEW RESEARCHES ON THE DEEP ORIGIN OF THE GLOSSO-PHARYNGEAL, AUDITORY, FACIAL, ABDUCENT, AND TRIGEMINAL NERVES. By LAURA GIOVANNI BATTISTA (*Mem. della Reale Acad. delle Scienze di Torino*, 1879, and *Proc. International Medical Congress*, 1881).

1. The cells of the hypoglossal nucleus are provided with processes which pass into the roots of the nerve.

2. The fibres of the hypoglossal nerve that appear to arise from the raphé really spring from cells interposed between the raphé and the roots of the nerve.

3. The cells grouped along the roots and in front of the nucleus of the hypoglossal nerve (the anterior accessory hypoglossal nucleus of Duval) send their nervous processes outwards and backwards; only in a few cases could I demonstrate processes sent forwards along the root.

4. The cells of the *nucleus ambiguus*, Krause (the anterior accessory nucleus of the pneumogastric, Meynert) send their nervous processes inwards and backwards as far as the nucleus of the pneumogastric; they do not, however, then turn outwards in the roots of this nerve, but, on the contrary, turn inwards, forming the marginal fasciculus, which is found in front of the hypoglossal nucleus.

5. The nucleus ambiguus has an extent in the medulla equal to that of the classical nucleus of the hypoglossal, and is probably to be regarded as an accessory nucleus of that nerve.

6. In front of the roots of the pneumogastric nerve are large scattered cells which send their nervous processes, not into the roots, but forwards between the fibres of the white lateral column.

7. The cells of the so-called external auditory nucleus send their processes, not into the roots, but inwards and forwards into large fasciculi, which cross the facial nerve and go towards the raphé.

8. The cells of the facial, abducent, and trigeminal nuclei are provided with processes which pass into the respective nerve-roots.

9. As to the small cells of the gelatinous substance of Rolando, enclosed by the ascending root of the trigeminal nerve, my researches have given negative results; but I have demonstrated that the middle-sized cells found here send their processes inwards, and never into the root.

10. In the whole medulla oblongata, from the glosso-pharyngeal to the trigeminal nerve, there occur very large scattered cells, which send their processes towards the raphé and backwards. I could never follow them into the roots of any of the cranial nerves, as described by Meynert.

ON THE RELATIONS OF THE NUCLEUS TÆNIAFORMIS  
WITH THE OLFACTORY NERVE. By PROFESSOR RAN-  
DACIO.

(*Proc. International Medical Congress, 1881.*)

1. The claustrum (nucleus tæniaformis) is of very various forms, but in general follows the undulations of the Island of Reil, and may be compared with the squamous portion of the temporal bone of a fetus presenting a spur in the position which would correspond with the zygomatic apophysis. It often passes in part into the cortical substance of the narrow convolutions of the island, together with the grey matter of the inferior frontal convolution, of the upper temporal and with the lenticular nucleus, excepting where this presents its caudate extremity in zigzag (cauda nigra), which disappears on the outer side of the capsule of the pes hippocampi, and which, as well as the spur, seems not to have been previously observed by others.

2. The elements of the claustrum are identical in shape with those of the fifth layer of the island as observed in part by Meynert, but the fresh cells are furnished with a granular protoplasm containing a large nucleus with a nucleolus. This nucleus separates very easily, and is furnished with one or two poles continuous with nerve fibres, as observed by Luya.

3. The claustrum and amygdala may be taken as two points of concentration for olfactory impressions, to which is annexed another grey body, which I term the "hood of the amygdala," and which has, I believe, up to the present not been described.

4. The external and median fibres converging to the above-stated points (like appendages) follow the course of the afferent fibres, whilst they emerge as efferent fibres to pass into the cortical substance at the origin of the temporo-sphenoidal convolution, the seat of olfactory perception.

5. From this centre the nervous fibrils spread in different directions, thus :—the most superficial join the commissure and arrive at the posterior extremity of the corpus callosum. The deeper, if they do not form, join the taenia semicircularis on one side as far as the acies, and the posterior pillars of the fornix, on the other side, and so continuing together into the anterior pillars, they recurve in the corpora albicantia to pass into the cerebral peduncle, and from this point to the pons Varolii, until they return as far as the funiculi siliquæ in company with the facial rather further back than the glosso-pharyngeal or trigeminal, in the medulla oblongata, where the respective nuclei of these nerves are found.

6. There are some cases in which, on the one side or the other, one may see in the rostrum of the corpus callosum, some bundles of the median and external roots of the olfactory nerve, and sometimes joined to the striæ of Lancisi. This is what is found in many Mammalia, especially in the seal.

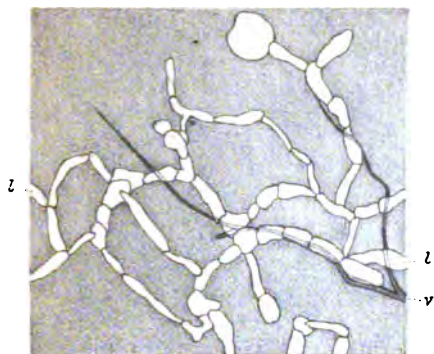
Contrary to the assertions of Luys, we must remember that the anterior nucleus of the optic thalamus has no connection with the

above-named olfactory roots (1) because the connection of the fibres is not in any way proved ; (2) because in a recent case of glioma in the third ventricle, not long ago diagnosed by Professor Federici, the perception of smell still existed in spite of the compression and partial atrophy of the optic thalamus, with destruction of the greater portion of the inferior layer, and of the crust of the left cerebral peduncle.

In the same way, despite the opinion of Luys, the internal root of the olfactory nerve has not its nucleus in the septum lucidum, for in two cases where this septum was missing, the sense of smell was unimpaired.

8. From the above-mentioned anatomical facts, you may deduce that the psychical centre of the olfactory sense resides in the "origin" of the temporo-sphenoidal convolution, in the region of the gyrus hippocampi. In this the researches of Ferrier find their principal support.

*Fig. 1.*



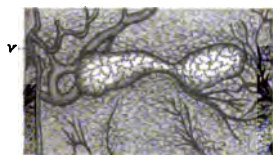
*Fig. 2.*



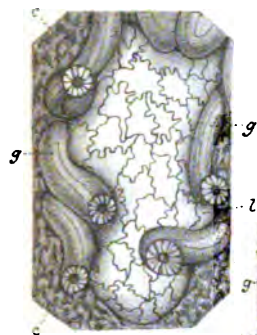
*Fig. 6.*



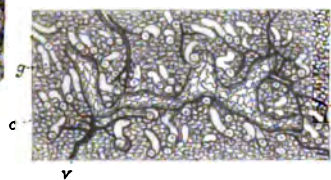
*Fig. 5.*



*Fig. 3.*



*Fig. 4.*

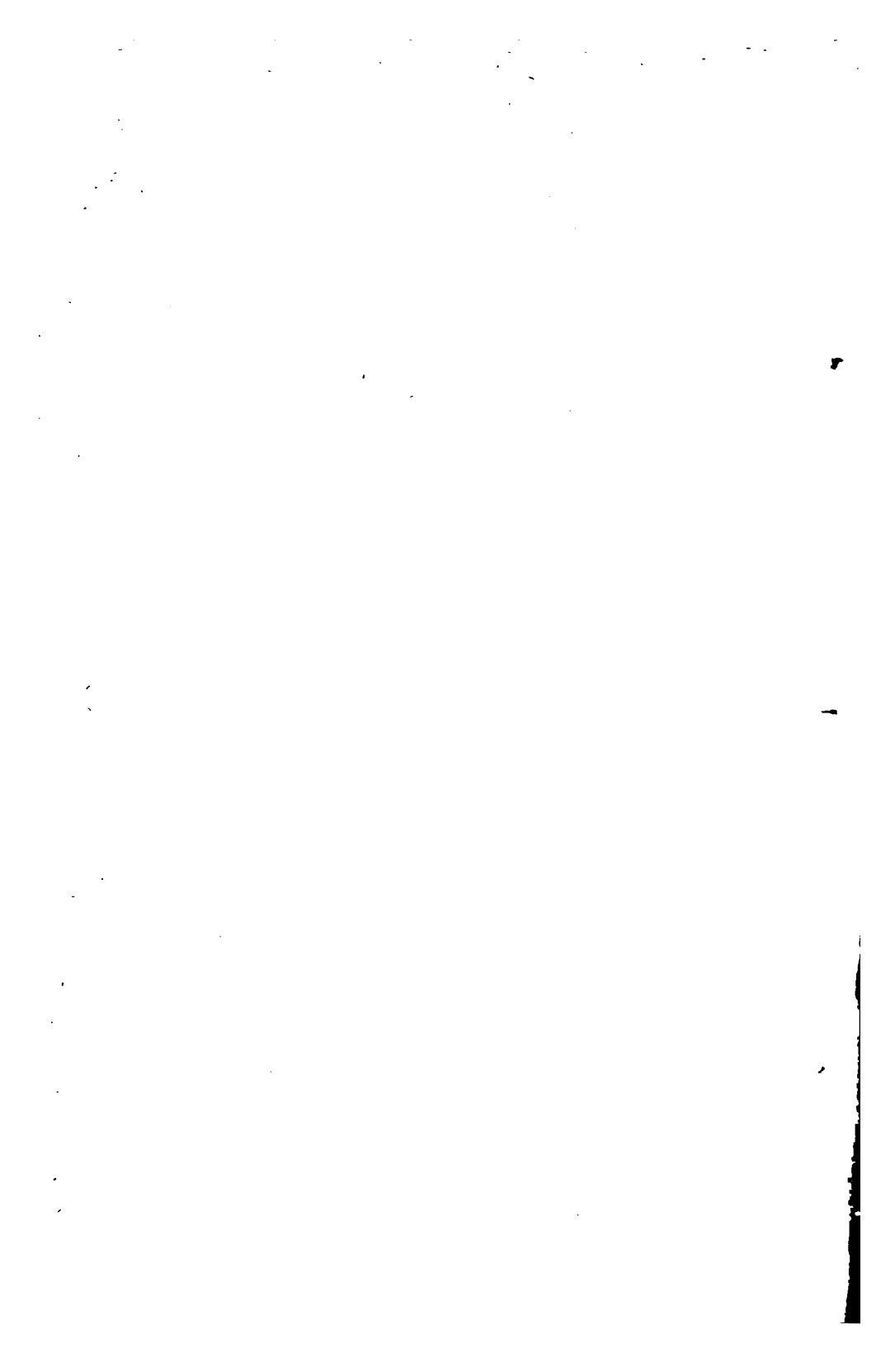


*Fig. 7.*



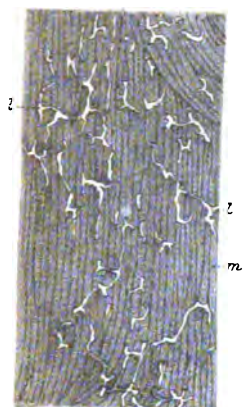
*Fig. 8.*



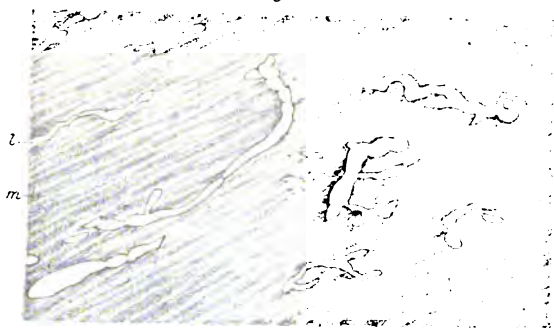




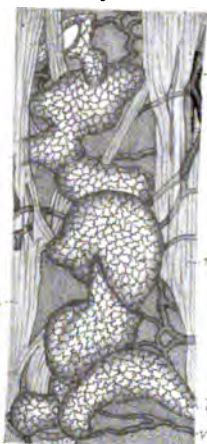
*Fig. 9.*



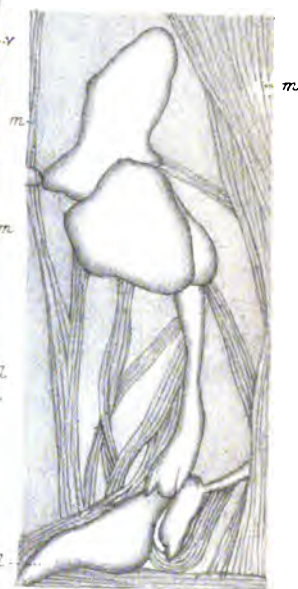
*Fig. 13.*



*Fig. 14.*



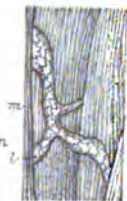
*Fig. 15.*



*Fig. 11.*



*Fig. 10.*



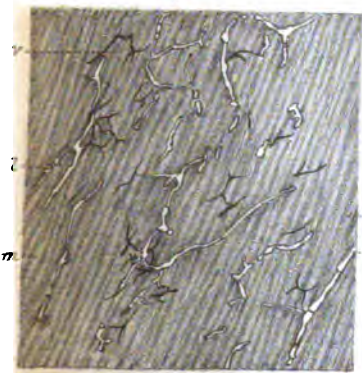
*Fig. 19.*



*Fig. 16.*



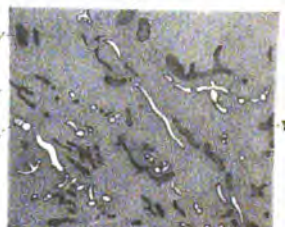
*Fig. 12.*

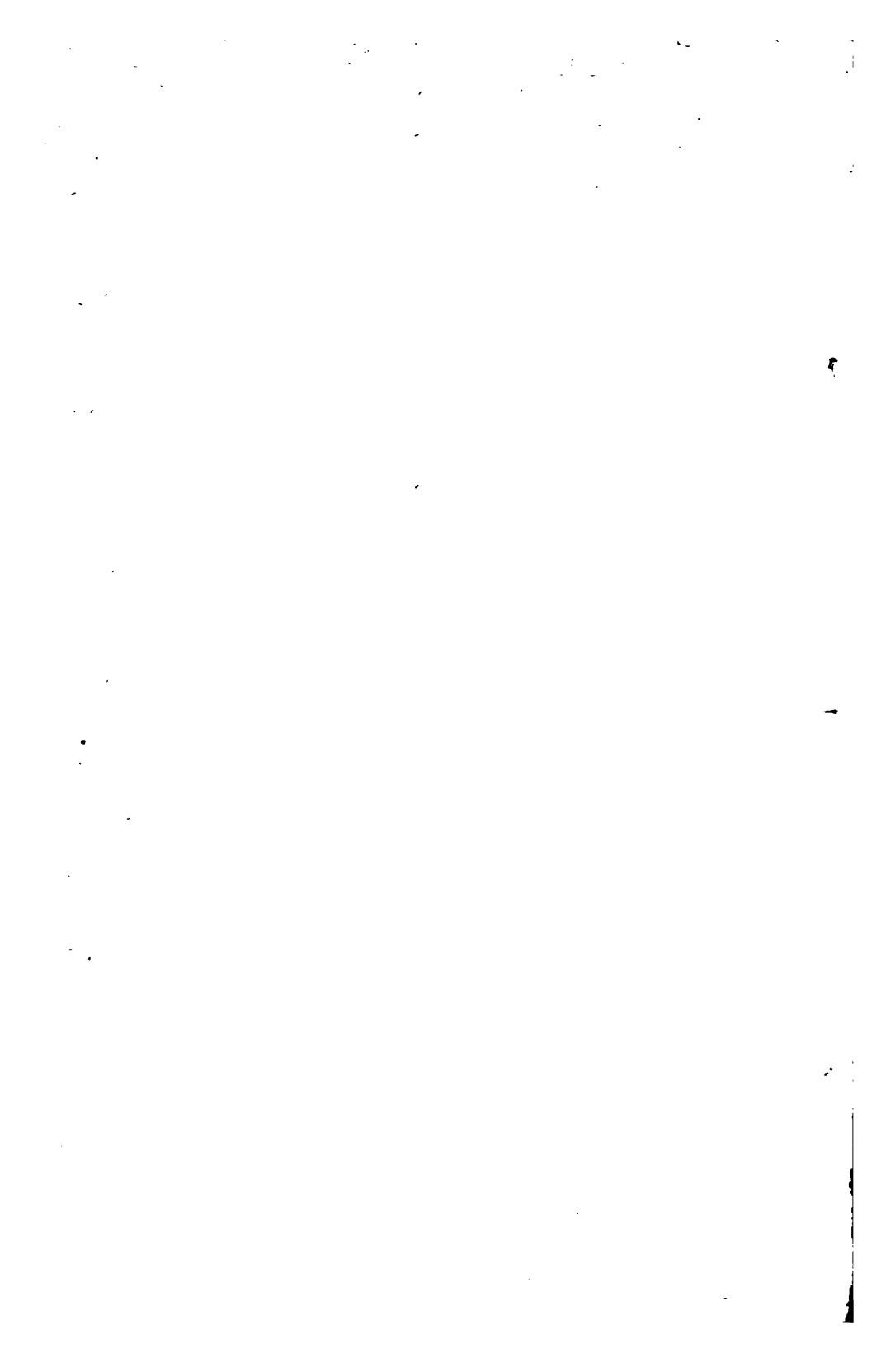


*Fig. 17.*

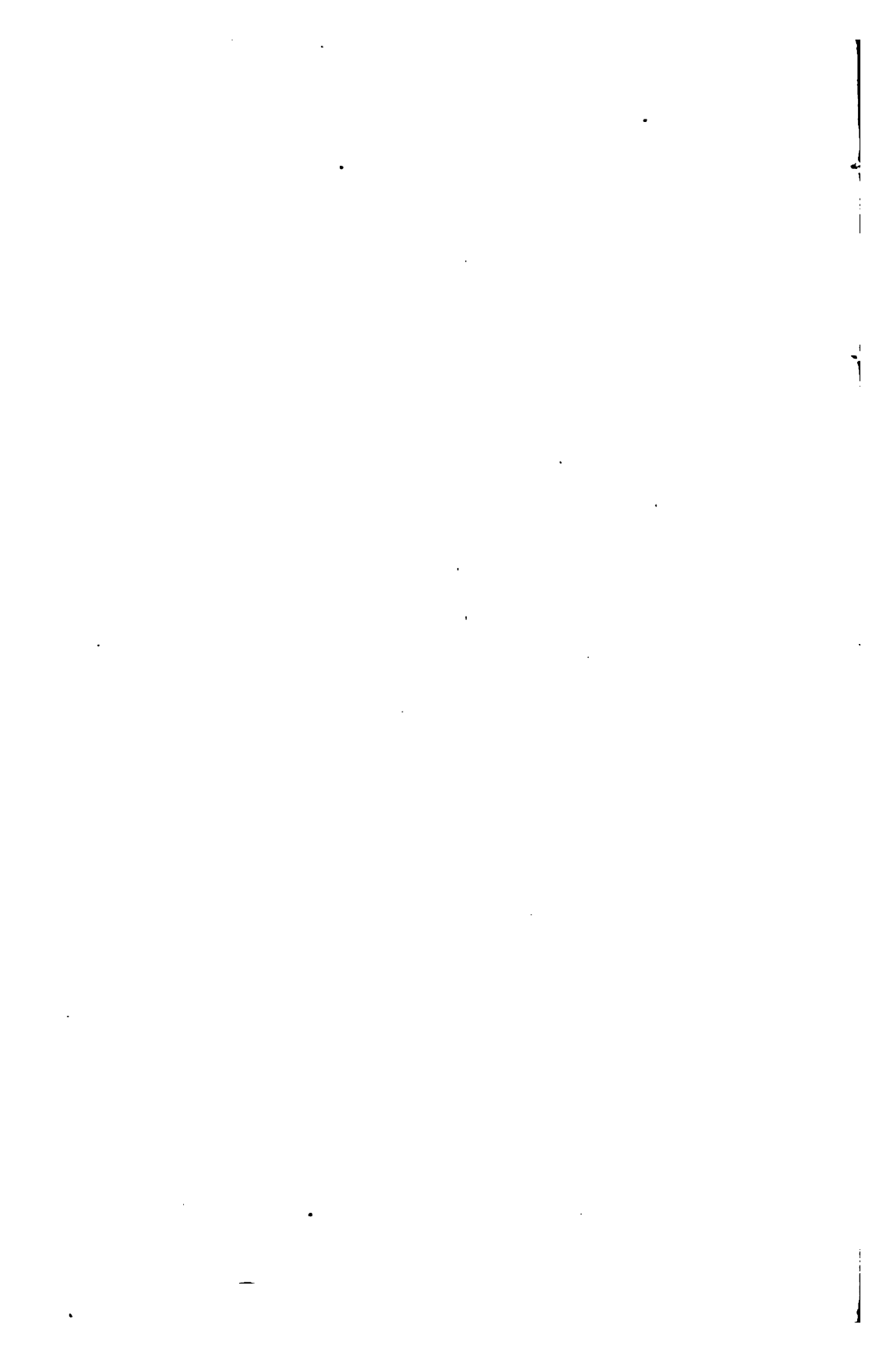


*Fig. 18.*

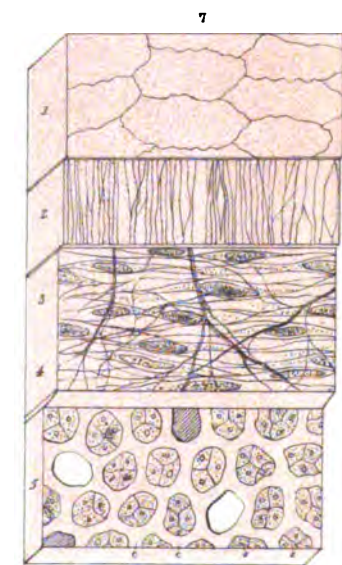


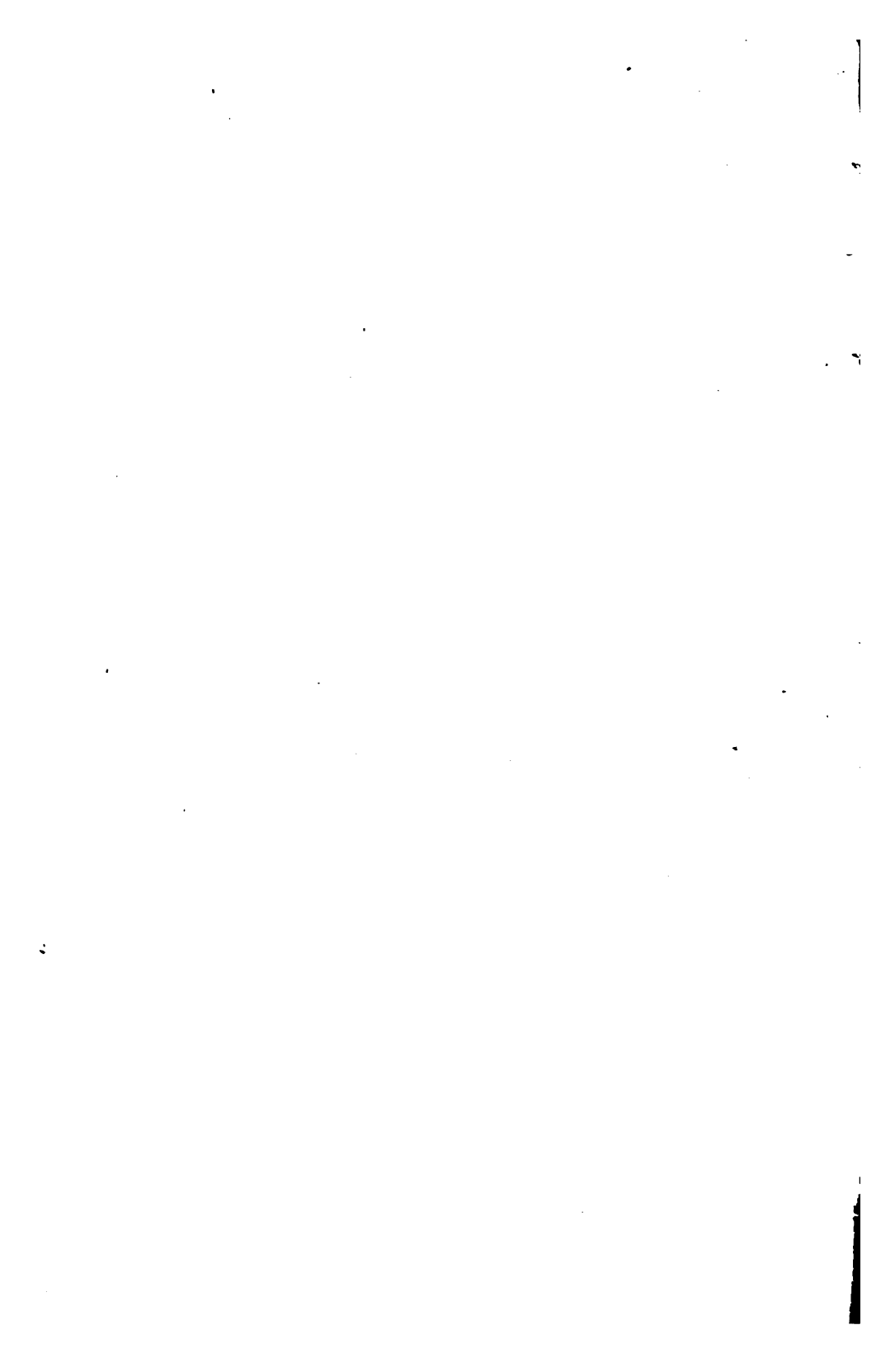


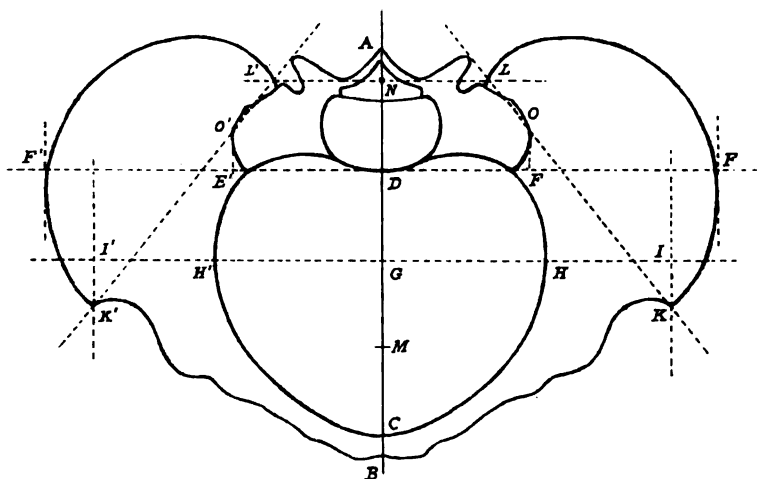




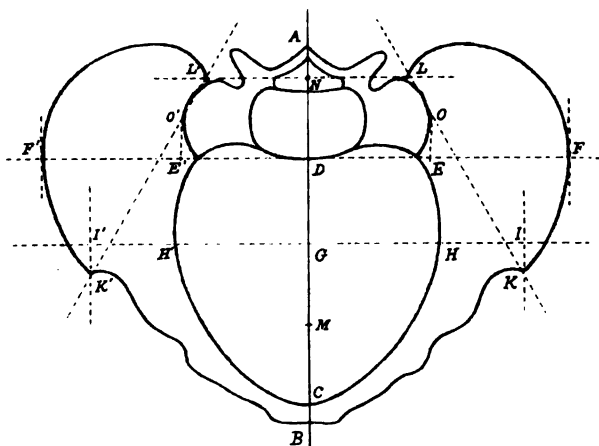




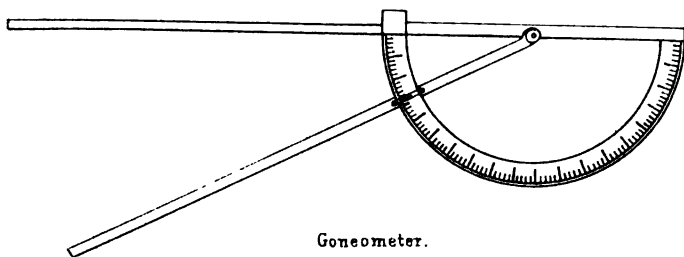




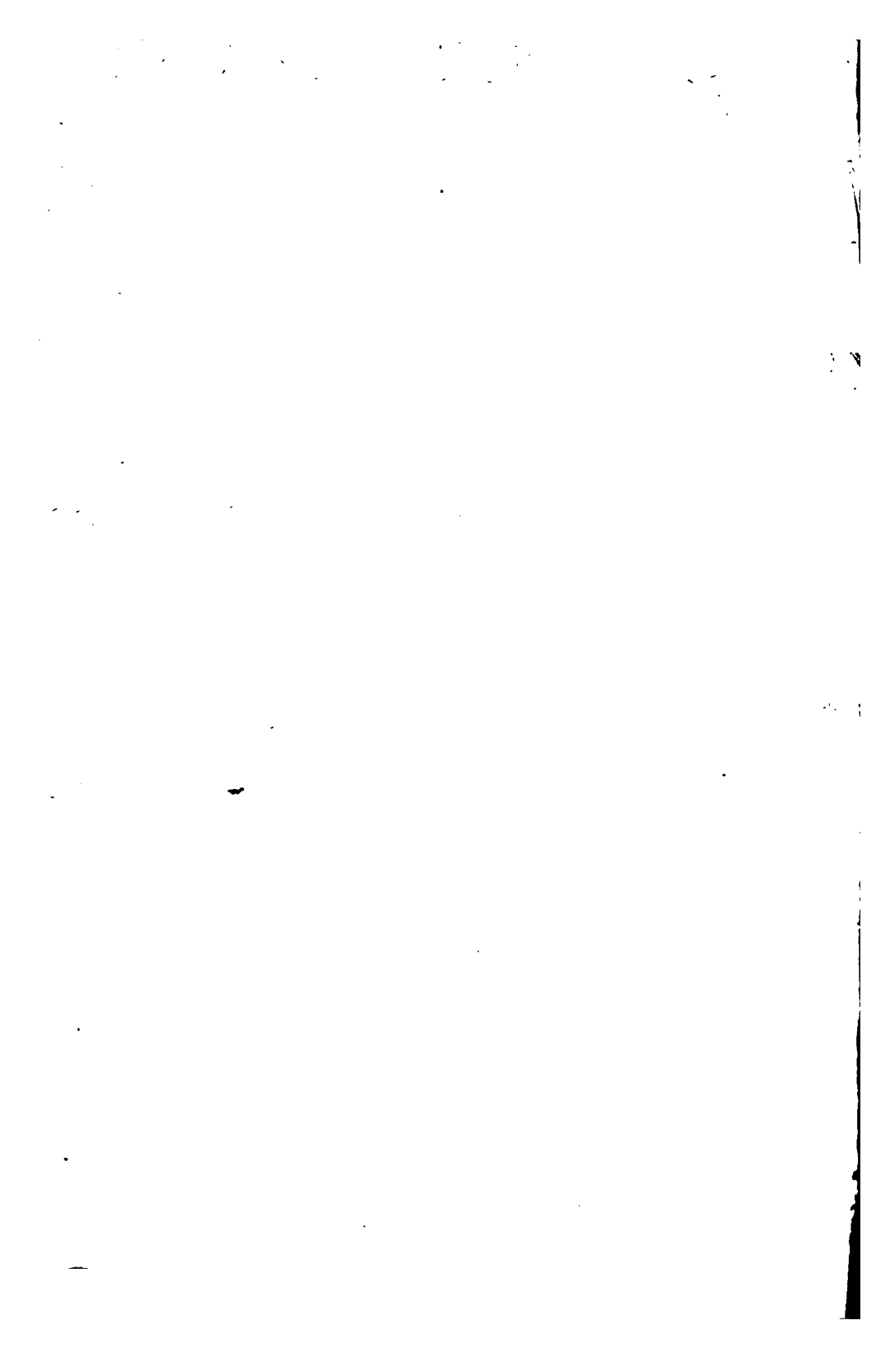
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Goniometer.





## Journal of Anatomy and Physiology.

OBSERVATIONS ON THE HISTOLOGY OF FRACTURE  
REPAIR IN MAN. By J. GREIG SMITH, M.A., *Surgeon*  
*to the Bristol Royal Infirmary.* (PLATES VI. and VII.)

IN 1853 Sir James Paget wrote<sup>1</sup>—"I cannot tell the conditions which will determine in each case the route of development towards bone that the reparative material (in fractures) will take, nor in what measure the differences that may be observed are to be ascribed to the seat or nature of the injury, or to the conditions of the patient. All these things have yet to be determined; and I believe that years of patient and well-directed investigation will be requisite for them."

The state of our knowledge on this subject is to-day but little advanced from what it was thirty years ago. Opportunities of studying the repair of fractures in man are so rare, and the modes of preparing the tissues for histological examination are so tedious and troublesome, that it is not surprising that few have directed close or prolonged attention to the subject. Such investigations as have been recorded refer almost entirely to fractures in the lower animals, which, as Sir James Paget has pointed out, but others seem to have forgotten, are to be taken only in a limited sense as illustrative of the process of fracture repair in man.

Proposing to myself the line of inquiry suggested in the above quotation, I have, during the past three years, made careful studies of all the cases of uniting fracture which have come into the dead-house of the Bristol Infirmary. Those cases of fracture, numerous in every large accident hospital, which are caused by severe crushes or falls, and in which death takes place

<sup>1</sup> *Lectures on Surgical Pathology*, vol. i. p. 244.

in a few days from shock or other complications, are excluded from the list. Such cases, though they have their value as illustrating the very first changes common to all fractures, are of little use towards the study of special features of bone repair. A few specimens of fracture of from three to five days' standing (three of ribs, one of humerus, one of thigh, two of radius, and one of spine) were examined, with special reference to the amount and condition of the effused blood. These presented no peculiar features, and need not be more particularly referred to. The cases which were fully studied, and from which the material for the following paper was chiefly drawn, must be separately described. They were—

1. A museum preparation, injected, of a simple fracture of the femur of a child a year old. This preparation had been mounted for some years in spirit, with simply the soft tissues removed. When bisected longitudinally it proved to be a beautiful specimen, showing ensheathing and intermediate callus in the most perfect manner. There was scarcely any displacement, and the ensheathing callus, of spongy porous bone with its trabeculae arranged perpendicularly to the surface of the shaft, was of a regular ovoid shape. The intermediate callus was only partially infiltrated with bone salts. The specimen is a typical one of the normal mode of repair as seen in fractures in the lower animals, and fully bears out Sir J. Paget's supposition that in fractures of long bones in young children the process of repair is "very like that described from the experiments on animals." Half the section was used up for microscopic study, but the soft tissues, from prolonged soaking in spirit, were not in good preservation.

2. An oblique fracture of the tibia, extending into the ankle joint, in a man aged thirty-six, who died on the ninth day of delirium tremens. There was double comminution of the fragments, and the periosteum was completely torn through, except over a portion of the smallest fragment. The fissures were filled with blood-clot in various stages of transformation. The specimen was valuable also as showing an early stage of periosteal inflammation, and the first beginnings of calcification in the powerful ligament which unites the lower extremities of the tibia and fibula. For this specimen I am indebted to the courtesy of Mr Pickering, house surgeon to the Bristol General Hospital.

3. A case of simple fracture of the femur in a man aged sixty-one, who died of an acute attack of erysipelas twenty-one days after receipt of the injury. The fracture was moderately oblique, and there was overlapping to the extent of about an inch, as well as some rotatory displacement of the fragments. The exposed medullary canal was, in each fragment, plugged with partially decolorised blood-clot, and a mass of brownish red clot lay at a little distance from the line of fracture in the sodden and condensed muscular tissue. Blood extravasations in various stages of decolorisation extended for several inches above and below the line of fracture into the various muscular septa. The periosteum was everywhere completely torn through except where it covered the *linea aspera*; and here it remained intact, stretched between the displaced fragments, enormously thickened, and traversed by several spicules of calcified material. Tracts of cartilaginous-looking substance ran in various directions through the condensed tissues surrounding the fracture; at many points these tracts were impregnated with calcareous salts.

4. A case of fracture of the body of a lower dorsal vertebra in a man aged thirty, who died, a month after the accident, from injury to the spinal cord. A wedge-shaped portion of the body had been pushed backwards, leaving a gap between the upper and the lower fragments of the vertebra, which was filled with organised blood-clot. New bone, developed chiefly in the ligamentous tissues overlying the *vertebræ*, had almost completely filled up the irregularities produced by the displacement of the comminuted fragment. Very little change, beyond a slight condensation, had taken place in the cancellated bone through which the fissures extended.

5. A compound fracture of the tibia and fibula in a man aged thirty-five. The limb was amputated on the twenty-second day, on account of extensive sloughing of the soft parts on the front of the leg. The case was treated antiseptically, the temperature being normal, and the slough being absolutely free from odour throughout. The front of the tibia was denuded for 3 inches above and 2 inches below the line of fracture; and here there was no sign of callus material. There was a considerable development of reparative material between the two bones, and a small amount of the same behind the tibia. The material

surrounding the fibula afforded many admirable specimens of the process of repair. This preparation had a practical as well as a pathological interest, proving that under antiseptics at least we may get a compound fracture repaired in the same way as a simple fracture, and not necessarily, as Dupuytren and others supposed, through the medium of granulation tissue only. But such a proof is, in these days, almost superfluous.

6. A simple fracture of the tibia and fibula in a woman aged forty, who died of an attack of acute bronchitis twenty-five days after the accident. This was a typical case of repair, there being no comminution and little displacement. Except in the exposed medullary canals, where there were some remains of partially decolorised clot, all appearances of blood-extravasation had disappeared. The reparative material, fibrous, cartilaginous, and in parts bony, surrounded and glued together the severed extremities.

7. An intra-capsular fracture of the neck of the femur in a woman aged sixty-one, who died of bronchitis four weeks after the accident. The fracture was close to the head of the femur, and traversed the neck in a plane at right angles to its axis. The crevices in the spongy bone were filled with partially organised blood-clot. On the upper aspect a bridge of fibro-cartilage united the fragments, in the substance of which, but not continuous with the old bone, was a nodule of calcified tissue about the size of a pea.

9. A case of fracture of an upper dorsal vertebra in a man aged twenty-five, who died of disease of the cord eleven months after the injury. A fracture very similar to that in case 4 was found perfectly united by the development of osseous material in the overlying ligamentous tissues. There was thickening of the bony trabeculae along the lines of fracture, and partial calcification of the adjacent intervertebral fibro-cartilages.

10. A wet preparation in the Infirmary museum of a fractured femur in the adult afforded some good specimens of callus calcification, but was not fully examined.

Each of these specimens was prepared in various ways. Portions of the surrounding soft tissues, in which it was important to preserve the first appearances of calcification, were cut off and hardened in Müller's fluid, or in spirit. For decalcifica-

tion, chromic acid and various mixtures with nitric, hydrochloric, and picric acids, were used, parts of each being prepared in several ways for comparison. On the whole, the specimens prepared in a mixture of chromic and nitric acids were the best. Blood-clot hardens admirably in a two per cent. solution of chromic acid.

These specimens, though small in number, may be taken as fairly illustrative of the mode of fracture repair in man as it occurs under the direction of the surgeon. And this, I take it, is what medical men, practically concerned in the treatment of injuries, chiefly desire. In its legitimate position as a branch of comparative histology, the study of fracture repair in rabbits, dogs, cats, frogs, &c. has been of extreme value to the surgeon. But these studies in comparative histology have not retained their true position. In most works the repair of fractures in man is described as a heterogeneous generalisation of the varieties observed by various experimenters on different animals; and, with the material at hand, perhaps this was the best that could be done. The following paper is offered as a contribution towards the placing of our knowledge of the repair of fractures in man on an independent footing. Many links in the chain of events are unavoidably missing; it is only in an imperfect way that the appearances in one case complete the history of the changes in another. Particularly have I to regret this in the case of blood-clot, which in these times has come to possess a high interest in surgical pathology.

The development of the uniting medium in broken bones in man takes place in parts which are usually kept at perfect rest. We use every means to minimise the inflammatory process; but, under varying circumstances of injury at the time of, or unrest after the accident, we may get every grade of inflammation up to strangulation and sloughing. In children a moderate amount of movement, coupled with mechanical elasticity and active nutritive changes, may give us a mode of repair simulating that in the lower animals. Another variety may be seen in a fractured rib, where the periosteum is lacerated on one side and entire on the other, and where, also, there is always a certain amount of limited but continuous movement. The most common form of fracture is seen in the long bones of an adult, kept

at more or less perfect rest, and, so far at least as surgical pathology is concerned, we must look upon these as our most important fields for study. In most of these the periosteum is completely torn through, and the harassed question of subperiosteal callus is at once rendered subordinate. The displacement in man is measured by the inch, where in the lower animals usually experimented upon, it would be reckoned perhaps by the line. The elements of repair in ultimate size and strength are identical; and the problem has to be solved, as an engineering one would be where a different strength on a different scale of magnitude has to be secured with a similar material. All these conditions involve an endless variety in fracture repair. Ultimately, however, the whole study resolves itself into the capabilities of the various tissues surrounding a fracture, to develop new bone. In each individualised tissue there is a certain course of events which, within certain definable limits, will be constant. In the pulpy coherent mass which surrounds the fractured ends we can recognise the fibres of periosteum, muscle, and areolar tissue, traversed perhaps by tendons and vessels and nerves, and bound closely together by coagulated blood and inflammatory exudation. Roughly speaking, this is the matrix in which the bony reparative material is to be developed. New embryonic tissue is introduced; old tissues are inflamed, and revert in varying degrees to their embryonic types of structure; and, generally, where any special function is resident in a tissue, we shall have exaggerated display of that function. The question is thus a double one—the degree of conversion to embryonic form which a tissue will undergo; and the amount of ossification or calcification which it will tolerate. In each fracture these degrees or amounts will vary, the changes in one stopping short of what they will reach in another. The descriptions will be given as if the process were completed in every instance for each tissue.

*Blood-Clot.*—The first question relates to the disposal of the blood which, in varying amount, is poured out into the bruised and lacerated tissues. Hunter thought that all callus material was derived from blood-clot. Amesbury and Dupuytren described it as becoming firm, coherent, and ultimately organised in the exposed medullary canals and between the separated

fragments. Paget says that blood-clot may organise in fractures, but that, as a rule, it completely disappears, to be replaced by the material fibrous, fibro-cartilaginous, and cellular, in which the new bone is to be developed. Billroth leaves it an open question whether it is entirely re-absorbed or becomes partly organised.

Undoubtedly, especially where the extravasation is abundant, a great portion of the blood is absorbed, and in no way helps repair; but it is equally true that, in certain positions and under certain conditions, not only does blood-clot become fully organised, but it may serve as a nidus or parent tissue in which the new bone is developed. Under certain circumstances, as we shall see, it may be the most important means of repair in the fracture.

Objection has been taken to the term "clot organisation." Of course blood-clot does not exist in the organism as a permanent tissue, and in this strict sense the term is a misnomer. But it is certainly true that clot may exist in the body over a period of time and under conditions of vitality which justify its elevation for the time to the dignity of an independent tissue. In this sense the term "organisation," in default of a better, may be admissible. A further justification of the use of the term will be given in the sequel, when its peculiarities as a specialised tissue will be described.

Blood-clot is an unstable structure, very prone to become disorganised or absorbed. What conditions determine the permanency of clot in fractures? It would appear that perfect rest at least is necessary to clot organisation. It is found most constantly in protected positions—filling up the jagged hollows in the fractured bones, plugging the medullary openings, and occupying the spaces between comminuted fragments. In these positions it is more or less perfectly protected from muscular and other movements. In fissured and impacted fractures I have always found it present. It is a matter of practical experience in antiseptic surgery that rest is necessary to clot organisation. Rough handling, or a momentary separation of the clot from the outlying tissues, is enough to cause subsequent disintegration. It is always easier to get a clot to organise inside a bony cavity than in soft tissues. Once the clot sub-

stance is disturbed it is very unlikely to recover itself. One reason why fractures of the neck of the femur so seldom unite, I believe to be that the clot between the fragments, which is the only medium available for union, cannot be kept at rest, but is rubbed off and broken down. In a fractured vertebra or skull bone there is little movement, and union will in time take place.

It would seem also that a moderate and not excessive degree of vascularity in the surrounding tissues is necessary for clot survival. In fractures, one reason why we find clot organising by preference over the bones and not in the soft tissues may be that the bone is not so vascular. The process of revascularisation is necessarily much more tedious in bone than in the soft tissues; the areas available are smaller, and the formation of new vascular channels slower. So we find it in practical surgery. In a bad case of excision of the elbow under my care, I tried to fill a large abscess cavity over the joint with blood-clot, so that a greater amount of new tissue less likely to form contractile cicatrix might be left to cover the new joint. The clot organised and bled on scratching on the fourth day; but, as inflammation came on in the surrounding abscess wall, the periphery of the clot broke down while the central mass lived and became decolorised. It was only after the clot became fully vascularised that it united with the surrounding tissues, and this it did by granulation. It is also a matter of experience that we are not likely to get clot organisation inside a cavity that has been scraped or washed out with a powerful chemical fluid—that is where the resulting inflammation is intense. Generally speaking, in simple aseptic wounds which heal without irritation, there can be no doubt that the uniting medium is blood-clot rather than “coagulable lymph.”

We may therefore say, with some degree of confidence, that where blood-clot is most perfectly protected from mechanical disturbance, and where it is least liable to be invaded by excessive vascularisation from the surrounding tissues, there it is most likely to survive and become organised. It need scarcely be added that deficiency of vascular supply is likely to be as fatal to its survival as excess thereof.

We take up the organisation of clot at the point where ordi-



nary coagulation of blood outside the body ceases. The fibrin has formed a regular and delicate honey-combed network around each cell; this network towards the upper surface of the clot becomes coarser in texture, and is gathered into broader strands with fewer embedded cells, till, when the surface is reached, the fibrin, with more than its due proportion of leucocytes, is the chief constituent of the clot mass. Here there is no evidence of cellular activity. If there is any cellular change at all it is in the direction of decay, not of vitality.

In clot that is to survive, the first change to be noted relates to the disposition of the fibrin. The regular arrangement around individual blood-cells still remaining, but in attenuated form, there is observed at points in the clot substance a tendency to the formation of leading strands, which may become many times thicker than the original fibrils (Pl. VI. fig. 9, *c,c*). In the immediate neighbourhood of these strands appear rounded cavities, each containing a deeply stained rounded cell with granular contents and a prominent nucleus (*a,a*). These are white blood-cells. They are here among the fibrin presiding over their special areas of nutrition, and directing the first steps of the tissue in which they lie towards vital organisation.

These leading strands, as well as the delicate network which they traverse, always stain readily. But the embedded blood-cells retain their yellow colour for a longer period, and, unless soaked for a considerable time, refuse to absorb the staining material. After a time, on the appearance of certain large granular yellow cells to be presently described, the blood discs are decolorised, and at the same time appear to lose their distinctive shape, becoming more or less spherical. In the end it would be impossible to say that the clear highly refracting substance traversed by delicate honey-combed fibrils had originally been red blood-cells. In the meantime the collection of the fibrin into leading strands has gone on, the large ones becoming larger, and new ones appearing. By contrast, as well as in reality, the original honeycomb network now appears to be of extreme tenuity (Pl. VI. fig. 3). The leucocytes undergo little change in their cavities. Here and there evidences of fission can be detected, but these are less frequent than might have been expected. They are always surrounded by a clear space,

appearing to fill only a part of the cavity provided for them. This may be an effect of the mode of preparation.

At this point the clot consists of fibrinous trabeculæ which give off smaller and smaller branches, enclosing areas of the original but much attenuated network, and having scattered throughout, but chiefly near the trabeculæ, numbers of the above mentioned rounded cells. Up to this point it may be said that the progress of organisation is uniform. But hereafter divergences take place which demand separate consideration. At every step we are reminded of the many-sided nature of clot tissue, ready, according to the situation in which it is placed—we may say according to the manner in which it is infected—to mimic almost any form of connective tissue.

The first change falling to be described relates to the appearance of round spaces, each surrounded with a row of large epithelioid cells, and containing a clear gelatinous substance which usually falls out on making the section (Pl. VI. figs. 2, 4, 5). These spaces usually occur in clusters, several large ones, with a good many small ones, being grouped together. In every direction of cutting they come out circular, therefore they must be hollow spheres. The largest ones, always few in number in a given section, can be seen with the naked eye, but by far the greater number are microscopic. They are most abundant in the central portions of clot, but they are by no means confined to these parts.

This process of alveolation—for such it practically is—is peculiarly interesting and beautiful. In all the specimens which I have studied, the steps of formation were strikingly similar.

The first evidence of alveolation is a grouping together of three or four epithelial-looking cells, with granular contents and a well-defined nucleus (Pl. VI. fig. 1). These cells are at first rounded in contour, but present flattened faces towards each other. They multiply excentrically, encroaching on the clot-tissue outside, and leaving a cavity inside. In size they vary greatly, but are always large; while a few in the largest cavities attain to enormous dimensions (Pl. VI. fig. 5). They are more or less flattened, adhering by their broadest surface to the wall of the cavity. Each cell contains a round nucleus

of very constant size—about that of a blood-disc. Some cells are vacuolated, and many contain drops of fat. They lie with great regularity, and usually in a single row on the wall of the cavity. In all stages of growth they are yellow in colour. They take on staining, not very readily however, with logwood, aniline blue, or carmine.

As alveolation advances the network forming the cavity wall becomes more dense and closely set (Pl. VI. fig. 5). When alveolation has gone to its full extent, the clot-tissue consists simply of the close-meshed network of thick fibres which forms the walls of the cavities with its embedded leucocytes and the enclosed granular cells and gelatinous material. One of my specimens actually shows complete calcification of these fibrinous festoons. Rarely, however, do the changes go so far as this.

To help our understanding of this process some analogous appearances may be described. Where blood-clot overlies yellow marrow we frequently find rounded masses of pure fat, usually much larger than ordinary fat-cells, which appear in their aggregation to have displaced the clot-tissue proper. Fig. 16 is a drawing of such a condition, in which the fatty aggregations are small and the clot-tissue has become calcified. Similar masses of fat may nearly always be found in clots formed outside the body, and especially in clots that are swollen from having been hardened in too weak solutions of chromic acid. There is no appearance of cells around such cavities; the process probably depends on the general tendency of fatty molecules to gather themselves into larger and larger masses.

But, although the processes are, thus far, dissimilar, they may be to some extent related. The crude aggregation of fatty molecules outside, or, where there is excess of fat, inside the body, may, under ordinary circumstances, in the living clot, be represented by a more elaborate segregation and collection. I cannot be certain that the contents of these cavities are pure fat, but I believe they are fatty in nature. They are probably to be looked upon as an early effort of the clot-tissue to differentiate its component elements. Their never getting beyond the condition of hollow spheres, and the

nature of their contents, forbid any supposition that they may be vascular in nature. They are probably rather destructive than formative in their tendency. Their usual position, in the part of the clot furthest removed from blood-supply, and their universal tendency, if not arrested, to eat up the clot-tissue proper, point to such an interpretation. They exist by reason of the peculiar instability of clot-tissue, ready to be preyed upon by any special activity which may lay hold of it.

But this process of alveolation rarely invades more than one-eighth or one-tenth part of the organising clot; and even when it has advanced to its farthest extent, it may leave behind it a tissue which, as we shall see, takes its share in the repair of the fracture.

Another element in the tissue fabric is a large yellow granular cell (Pl. VI. fig. 6). Free of any connection with the tissue structure, and standing out of yellow colour in the midst of the blue or violet field, it is a striking object in most preparations. It is about twice or thrice as large as a leucocyte, is devoid of a nucleus, and is contained within a well-defined cell-wall. It usually appears about the third or fourth day, and disappears again about the eighth or tenth.

I think there can be no doubt that this cell is concerned in the collection of the colouring matter of the blood from the extravasated blood-discs, and perhaps even in the disintegration of the discs themselves. There need be no question as to their yellow granular contents being the same as that of the blood-discs; and the appearances often seen inside the cells lend a high probability to the view that they also lay hold of many of the discs bodily, and break them up in their interior. In this sense they are, in the fullest meaning of the word, hæmatoclasts, analogous to the large granular yellow cells of the spleen and other blood-granular bodies. We can guess that they are leucocytes to which this new function has been relegated; what ultimately becomes of them I cannot say. It is not unlikely that they, like their splenic congeners, are fated to help in the manufacture of new blood-discs, this function being long in abeyance owing to the tardy process of vascularisation in clot. They are certainly found in greatest abundance around areas of special activity which are to be the seats of vascular for-

mation ; but I cannot say that I have found direct proof of their hæmatoblastic function in the way of fission or budding. Such a function is highly probable, but cannot be described as certain.

At this stage we may take up the vascularisation of clot. And here I may at once say that I believe there occurs in clot organisation an independent and original formation of blood-vessels, similar to what occurs in some other embryonic tissues, such as inflammatory exudations, sarcomata, placental tissues, &c. *A priori* we might have suspected this. This special vaso-formative function being found in rapidly growing tissues of simple type, apparently as a condition of their existence and growth, we might have reasonably enough concluded that a blood extravasation—the most rapidly produced of all tissues, and apparently the least stable—would be endowed with similar aid to survival. The preformation of blood-channels in the clot-substance, ready to be joined on to the general circulation, must very materially contribute to the rapidity of its organisation, and thereby to the probability of its survival.

The vaso-formative process is seen at its simplest in clot which is at a distance from any heterogeneous tissue such as bone, ligament, or muscle. Here it is seen to consist essentially in the collection into a tubular form of the protoplasmic or fibrinous strands which traverse the tissue in every direction. A chance parallel arrangement of a couple of thick trabeculæ must not be mistaken for the formation of a vessel. In such a case the original network remains between the thick trabeculæ, and there is no true tubular formation. When a vessel is to be formed the matrix gathers itself round a central axis to form a tube (fig. 9, *b*), and the fine cellular network is pushed aside. In this condition the young vessels bear a striking resemblance to the venous radicles of the spleen (figs 10 and 11). A vascular territory is one of special activity, and is a marked object in the field from its deeply stained appearance and its abundance of cells. The suggestive meaning of the crowding of large granular yellow cells in these areas has already been alluded to.

But the most active element in these vascular areas are those round deeply-stained cells already described as each lying in a fibrinous nest. An evident purpose in their multi-

plication and arrangement can now be made out (Pl. VI. figs. 10 and 11). They lie at commanding points among the chief strands, foreshadowing in their own arrangement and in that of their subsidiary trabeculæ the formation of the young vascular tubes. They are always round or oval, are usually very deeply stained, and not infrequently exhibit a small dark excentric nucleus. These are the true angioblasts in clot-tissue.

Other forms of vascular development which are met with are probably predetermined, either by the tissue into which the blood has infiltrated or to the inflammatory exudation with which part of the clot may be commingled. Fibres of periosteum or ordinary areolar tissue are sometimes found traversing a clot in all directions, importing new elements and new influences. Therefore, though here also the vaso-formative process may be independent of the general vascular system, it would be wrong to describe it as purely inherent in clot. A common form of vessel growth is drawn in fig. 12, from the margin of a clot. Figs. 13 and 14 are small portions of a similar tissue in other sections in which the vaso-formative action was resident. Here there is nothing peculiar to be noted; and the drawings sufficiently explain themselves.

The young clot tissue thus endowed with potentialities of independent existence, assumes many peculiarities of form. Some of these changes, appearing, as they do, to bear important relations to the process of fracture repair, require special notice.

One of the most important of these changes, pregnant with a far-reaching suggestiveness as to the capabilities of blood-clot in fracture repair, is a transformation into a tissue which exactly resembles pure cartilage. Fig. 7 is a drawing of such a condition. There is no possibility of mistaking the appearances. There is a distinct gradation, more marked in the actual specimen than in the drawing, between the familiar young clot-tissue, with its vaguely striated trabeculæ and round cells, to the glassy matrix and encapsuled cell groups of the cartilage. The clot fibres, becoming more and more swollen, are ultimately transformed into a continuous matrix, while the encapsuled cells multiply in broods round a mother cell and become surrounded singly or

in groups with new matrix. The resemblance to cartilage here, at the apex of the transformed tissue, is complete enough, but lower down it was even more perfect, and underwent an infiltration with calcareous salts in a manner no way different from what is seen in ordinary calcification of cartilage.

This was a change striking enough to deserve most careful investigation, and the appearances were closely observed in every specimen. One point to be decided was, What determined the transformation? for it was scarcely conceivable that blood-clot *sua sponte* should thus specially prepare itself for becoming calcified. It was noted that such cartilaginous areas ran into the clot substance on the side next the bone, and that they could often be traced down to a bed of fibro-cartilage representing a muscular insertion. It seemed, in fact, as if the inflamed cartilage had first infected the inflammatory neoplasm in its neighbourhood with its histological peculiarities, and that this infection, carried out into the young clot, had been immediately taken up by that versatile tissue. In nearly every case of fracture such cartilaginous tracts can be followed through the sodden tissues, and some of them can, with care, be traced to an area of coagulated blood. The infection is handed on from one tissue to another, and the transformation only ceases with the exhaustion of the stimulus, or the obstruction of a heterologous tissue such as muscle. That this tissue is pure cartilage I very much doubt; that it is a parent tissue specially adapted for the development of bone is perfectly clear. It is, furthermore the undoubted representative of that ill-understood tissue which is so abundant in fractures of the lower animals, and which has given us the name "callus cartilage." More will have to be said on this subject presently.

The most varied changes in clot are found in the sites most favourable to its existence, namely, plugging the marrow cavity and filling up the jagged hollows in the fractured ends.

Running into clot in these situations we always find numerous bony spicules or trabeculæ. The immediate neighbourhood of such spicules is always the seat of some special change in the clot-tissue. Fig. 15 is a drawing of one of the most common appearances. Trabeculæ are gathered together from outlying portions, meeting at the tip and lying alongside of the spicule.

Abutting on the bone the clot trabeculae are so closely arranged as to be almost continuous, and the imprisoned cells assume a rough similarity to the neighbouring bone corpuscles. The structure of true bone is, in fact, very closely mimicked. The assimilation is further carried out when this transformed clot-matrix becomes, as it frequently does, impregnated with calcareous material.

Some of the most striking preparations which I possess are derived from specimens in which ossification had invaded the alveolated tissue described above. Beyond the broad fact that calcification had invaded the clot trabeculae left surrounding the nests, and that the imprisoned cells closely resembled true bone cells, there was nothing very special to be noted histologically in these preparations. In these regions, the ossific stimulus being very near and powerful, every available matrix was laid hold of and calcified. Fig. 16 is a drawing showing calcification in the matrix around fatty aggregations, which are so frequently found where clot overlies yellow marrow.

It would seem that, given the adaptable leucocyte and the equally plastic clot-fibrin, there is scarcely a limit to the permutations and combinations that may arise therefrom. Even the sluggish articular cartilage is not without its influence on this versatile tissue. In case 2, which was only eight days old, the foreshadowing of a cartilaginous structure was observable in the clot all along the line of rupture of the articular cartilage. Fig. 8 is a drawing of such a condition, not very striking in itself, but eminently suggestive as to the ultimate capabilities of the young tissue.

It is scarcely possible to exhaust the multiple aspects of organised blood-clot in the midst of heterogeneous and inflamed tissues. The above description will perhaps suffice to show the nature and value of the young tissue in fracture repair. Wherever it lies it goes some distance towards assimilating itself to the tissues which surround or traverse it. If the ossific stimulus reaches it, it undergoes changes similar to those which would be undergone by the tissue it mimics. Even after making allowance for the unstable nature of clot-tissue, and for the manifold influences which militate against its survival, we cannot fail to perceive its importance in the healing process of fractures.



When no other tissue competent to develop bone is at hand, it steps in and fulfils this function; or when only heterogeneous tissues are present it meets them half way, and, bridging over the difficulty, carries the ossific stimulus on to another tissue which is competent to serve as a bony matrix.

But we must not exaggerate the value of clot in fractures. Clot is the least valuable of all repairing mediums. Its sluggish nature, hovering for days between life and death, and prone to destruction on the slightest disturbance; and the crude simplicity of its elements, far removed from that form of structure which is most suitable to development of bone, place it behind all the other tissues which surround a fracture as a medium of union. But there it is, for better or for worse, and we must accept it.

It is the last of all tissues to become ossified. Nearly all observers agree in describing the medullary plug as the last to develop bone. Through all its changes of decolorisation and subsequent vascularisation it has been described with a wonderful amount of uniformity by numerous observers; it had even been raised, under the name of "*substantia intermedia*" (Breschet), to something of the dignity of an independent existence. Evidences of its delay in ossification are seen in macerated specimens of imperfectly united fractures in most museums, where its previous existence is represented either by a complete gap or by very imperfect ossification. A practical proof of its drawbacks as an uniting medium is seen in fractures of the neck of the femur, where its friability and sluggishness render union almost impossible. Its existence anywhere in excess cannot be otherwise than deleterious.

Nothing need be said of the cessation of clot transformation at the stage of fibrous tissue. It is impossible to say whence the fibrous material so frequently found sealing up the displaced ends of a fractured bone derived its origin. It is just as likely to have sprung from clot as from anything else.

Here, then, we leave the process of clot organisation in fracture repair. Assimilated to surrounding structures either by calcification or fibrillation, it ceases to exist as an independent tissue, and its history is hereafter incorporated with that of its neighbours.

*Periosteum.*—As a rule, in fractures in man, the periosteum is

completely torn through. But the exceptions to this rule are somewhat numerous. In fractures of the ribs, for instance, the laceration is on one side only; on the other side, outer or inner, according to the mode of fracture, though partially detached, it usually remains entire. The same is true of fractures of the clavicle, and, with some limitations, of the fibula and radius. In fractures of the flat bones there may be no tearing of the periosteum, and only slight detachment thereof. And in many fractures of the long bones a few shreds of periosteum are frequently left stretching between the fragments.

When the periosteum has not been stripped from the bone at the time of the accident, it continues adherent during the progress of repair, and no amount of inflammation that is not suppurative will separate it. When it has been peeled off, as nearly always happens for some distance above the fracture, a sero-sanguinolent effusion, which has been described by most observers, lies between the periosteum and the bone. In considering the peculiar functions of periosteum in fracture repair, all these conditions must be remembered.

At the outset it is necessary to give a definition of terms. According to general usage, the term ossification may mean anything from the crude infiltration of a vitreous humour or of a pleuritic effusion with calcareous material up to the fully elaborated process as seen in the extremity of a foetal long bone; and calcification may mean almost the same thing. With such laxity of meaning it is impossible to make our terms understood. I shall therefore make use of the terms calcification and ossification in the following senses:—Calcification is taken to represent a simple deposit or infiltration of amorphous or nodular salts in a pre-existing matrix; the cells are the proper ones of the tissue, simply imprisoned and not specially developed. Ossification is the formation of a specialised tissue by specialised cells ("osteoblasts"), the protoplasmic being formed *pari passu* with the calcareous material; and true ossification is, in the first place, a sequel to the destruction of a previously calcified tissue. A tissue of calcification may never become ossified, continuing permanent or being completely absorbed. But an ossified tissue in its first development always succeeds a calcified precursor.

I am aware that the highest possible authorities might be cited in opposition to the above definitions. It is in sheer despair of making myself understood, unless some such rigid line be drawn between the two processes, that I so restrict the meaning of the terms. A correct appreciation of the differences between the two processes lies at the root of our understanding of the process of bony repair.

Thus defined, we see very little of true ossification in the early stages of callus development. It is nearly all simple calcification. The furious advent of the inflammatory process provides an abundance of succulent matrix and blood supply. In the presence of the ossific stimulus there is a hurried preparation for true ossification, consisting of an irregular but not altogether indiscriminate deposit of calcareous material in the inflammatory blastema. To some extent the matrix is an infiltrated product of inflammation, but the greater portion of it is derived from the swollen and otherwise transformed tissues around the fracture. One of the most important of these parent tissues is the periosteum.

Here it is expedient to interpolate a reminder on the periosteal function of bone production. The normal process of bony growth under periosteum is not, rigidly speaking, periosteal at all, but medullary. The periosteal osteoblasts are medullary in structure as well as in function. A slight periostitis, such as may be found under an ulcer of the skin, gives an increased development of bone, which is of the ordinary lamellar type, arranged regularly in Haversian systems. A greater amount of inflammation induces a new departure in osteogenesis. In this latter case the new growth is through calcification as a first step. The ready-made swollen periosteal fibres are made use of just as any other suitable matrix would be, and without any regard to its supposed function of osteogenesis, for the deposition of calcareous salts. The first is true permanent ossification; the second is only calcification of a fortuitously provided matrix, and is merely temporary. The one is analogous to the calcification of cartilage, which in the foetus precedes true bony growth; the other corresponds to the development of lamellar bone by medullary osteoblasts.

Looked at in this view, the whole question of the periosteal

origin of callus is much simplified. The function of the development of true bone being dependent on the sub-periosteal osteoblasts, and being medullary in nature, a normal ossifying periostitis is possible only when the periosteum is undetached and the osteoblasts undisturbed. But, as a matter of fact, we know that a detached or even transplanted periosteum carries with it a power not only of calcification but of ossification. All that we need in this case postulate is that some of the osteoblasts shall have been borne away with it. It is, however, a deeper question than the mere presence of osteoblasts; it is a question of degree of inflammation, anything between plastic growth and suppurative destruction. We find that the mean between these extremes is the hurried and histologically incomplete process of calcification. And experience shows that in fractures the degree of inflammation is in most instances enough for calcification of periosteal fibre, but too much for the direct formation of medullary bone.

The development of callus in periosteum presents a considerable variety of histological appearances. We find one prevailing type of growth in the more outlying portions of fibre, where it is less dense and more abundantly infiltrated with inflammatory material. Where it is in close contact with, and firmly adherent to the bone, another type of calcification is met with. Those instances in which the periosteum has been peeled off the bone and gelatinous material separates the two will be best included under this head. And, lastly, that more complete but less important development of bone, which is one effect of a low form of periosteal inflammation, and is produced by the periosteal osteoblasts, may be here considered.

Rindfleisch, while speaking of this subject, expresses his surprise that, according to Virchow's careful investigations, in ossifying periostitis bone may be developed outside the periosteum ("parostosis"); for periosteum ordinarily produces bone on its inner aspect.<sup>1</sup> In such cases I believe we ought to consider it only in a very limited sense as an "ossifying periostitis;" in a broader and more legitimate sense it is simply an "ossifying cellulitis," or, still more correctly, a "calcifying cellulitis." Where the cambium layer under the periosteum

<sup>1</sup> Rindfleisch's "Path. Hist." *New Syd. Soc. Trans.* 1873, vol. i. p. 244.

is destroyed, its specialised function follows suit; and any new function which the periosteum takes up must be in virtue simply of its capabilities as an inflamed fibrous tissue. As such, it may be infected with tendencies to calcification, but it is of itself incapable of developing true bone.

Of course it is not denied that a true ossifying periostitis might occur in fractures. The periosteum may be only slightly injured, and the resulting inflammation may be of a mild nature, and thus we might get an example of genuine periostitic bony growth. As a matter of fact, such examples are, in man at least, very rare. The limited capabilities of the cambium layer of osteoblasts are far from being equal to the rapid and excessive callus growth seen after fractures. Starting at innumerable points throughout the prepared matrix, and traversing many courses through the fibrous bundles, the calcifying process proceeds with a rapidity many times greater than could be brought about by a true ossifying periostitis.

Many experiments have been made on the lower animals with a view to teaching us the natural history of ossifying periostitis. Conspicuous among these are those by Busch, of Berlin, of inserting laminaria tents into the medullary cavities of bones. In these the periosteum was for the most part intact; and if we might deduce from an undisturbed condition of tissue a retention of functions, we should find it in such experiments. That is to say, we should have expected the new bone to be of medullary rather than of fibrous origin. The oncome of inflammation was slower than it would have been in fracture; and the inflammatory stimulus was handed on from the compact bone to the periosteum. Starting with an inflammatory hyperplasia of the medullary bone-producing elements, the new growth is laid down on the compact bone and grows outwards. Some slides in my possession, and for which I am indebted to the courtesy of Professor Busch, seem to show all this. Now, we know that in fractures a development of bone—or at least an infiltration with calcareous salts—often takes place at a distance from the old bone. In these specimens there is a striking similarity in general arrangement of trabeculæ to what existed in the callus developed in Case 1; but it was easy to make out fundamental differences in the processes of growth. The one was in all its chief features

a growth in membrane, the other a development by true osteoblasts. In that fracture also there had been but little or no laceration of the periosteum, and only slight displacement of the fragments. In fact, the periosteum had been left in a position histologically not unlike that in Professor Busch's experiments on artificial necrosis. We may contrast such a condition with the appearances depicted in fig. 1, Pl. VII., where true ossification and periosteal calcification are going on side by side, but not in unison. To appreciate the distinctions fully, decalcification by acids must have been very sparingly adopted; and the sections must not have been soaked in oil of cloves, nor mounted in balsam.

On the whole, therefore, I should characterise periosteal callus rather as a calcifying cellulitis than as an ossifying periostitis. We are to look for its true homologue in the membranous development of bone. The osteogenetic material which is always found at the tip of a calcifying spicule in ordinary intramembranous ossification is here represented by modified periosteal fibre (Pl. VII., figs. 2 and 3). Fig. 2 is a drawing of a section made transversely through the tip of a spicule. A zone of deeply stained protoplasmic material, which, before decalcification, had probably been infiltrated with nodules of calcareous salts, surrounds the substance proper of the spicule. This last appears as a granular matrix containing three branching and anastomosing corpuscles. At several points in the protoplasmic zone the amalgamation of round granular cells with the osteogenetic material is seen. Fig. 3 represents the tip of such a spicule undecalcified, and completes and explains the appearances drawn in fig. 2.

After this manner the first development of periosteal callus takes place. From many centres lines of calcification run along the bundles of periosteal fibre, leaving an areolar arrangement, whose leading trabeculae follow the course of the periosteal fibres—that is to say, parallel to the shaft of the bone. Between these calcified trabeculae there lies a succulent young tissue, with numerous large, rounded or tailed nucleated cells and a few fibres. Numerous blood-vessels of simple structure (fig. 4, Pl. VII.) traverse the young tissue in every direction. Occasionally some attempts at the lamellar development of bone by true osteoblasts

are seen ; but this is not common. The trabeculæ of periosteal callus show in their rough or granular structure the indiscriminate arrangement of their embedded cells, and the complete absence of anything like lamellation—wide differences from ordinary cancellous bone.

On peeling off the superficial, perhaps calcified, periosteal callus, we frequently find closely adherent to the underlying bone long ridges of hard glistening material traversing the strands of fibrous tissue. Sections in longitudinal direction can readily be made, though the tissues have not been decalcified. This dense calcified tissue is simply periosteum, the bundles of which have not been separated by injury or inflammatory exudation, and which have been thoroughly impregnated *en masse* with fine calcareous material. The changes in the histological elements are so inconsiderable that the process might be likened rather to an impregnation with some hardening material, such as Canada balsam, than to an organic or physiological proceeding. The wavy bundles of connective tissue can be readily detected in the hardened substance, and here and there the connective tissue cells stand out with abnormal prominence in the field. On the surface of such ridges and at transitional points elsewhere, a more marked change in the fibre cells, similar in nature to a more complete transformation, to be described under another head, can often be detected. Curiously enough, I found that this calcareous infiltration occasionally followed bundles of perivascular periosteum for some little way into the pores in the compact bone.

Why this hurried and imperfect form of calcification should be found next the bone, where we might expect it to be most complete, is difficult of explanation. It may depend on lowered vitality of the periosteum itself. The layers next the bone may have suffered most (as is conceivable if the injury were direct), or the vessels running between periosteum and bone may have been blocked. It is scarcely conceivable that it may have been caused by a simple superabundance of calcareous material in the neighbourhood which is poured into the fibrous matrix before it has undergone those elemental changes which indicate the oncome of inflammation.

Where the periosteum is peeled off the bone we find intervening a pinkish gelatinous material which is, histologically, composed of the elements of inflammation mixed with blood corpuscles, and traversed by shreds of periosteum. In its ulterior changes this material may assume an appearance similar to some forms of callus cartilage. Rokitsansky describes this gelatinous material as of constant occurrence in the lower animals. I regret that I have had only slight opportunities of studying it. Either by absorption or by ossific invasion it is soon lost sight of, leaving behind it a trabecular calcified growth which is very similar to that found in periosteum generally.

On careful examination of the new material under a stripped periosteal membrane, groups of nodular osteophytes may be found here and there closely adherent to the underlying bone. These sedentary osteophytes I believe to have a similar origin to the spicular or lamellar osteophytes found in the substance of subperiosteal sarcomata.<sup>1</sup> That is to say, I believe them to be partly periosteal, partly medullary in origin. The periosteal tissue is usually the adventitia of a vessel which has escaped laceration; the medullary tissue is derived from a vascular opening in the compact bone. The apex of the nodule or spicule is developed in the periosteal fibre, and is solid or areolar. The base is developed from medulla in the ordinary way, by alveolation and redeposition, and is hollow, and arranged in concentric lamellæ. The periosteal fibre leads the way, and develops the solid apex; the medullary tissue follows it up, alveolating and redepositing. These nodules, then, are little hollow cones attached by their bases to the periphery of a vascular aperture in the compact bone. Though they lie in the gelatinous subperiosteal layer, they are not, properly speaking, a development therefrom.

This pink gelatinous material is described as playing an important part in the repair of fractures in the lower animals. It has been spoken of as a special secretion for a special purpose. If we are to regard it as a secretion, it must be only in a limited sense, and with that antiquated meaning which regards even bone itself as a secretion. It is simply an inflammatory

<sup>1</sup> "The Mode of Growth of Spicular Osteophytes," *Bristol Infirmary Reports*, 1878-79.



exudation poured into an artificially formed space, and behaves in the manner that such a neoplasm lying between such tissues would be expected to behave. So far as I have seen, it plays an unimportant part in human fractures.

True periosteal bony growth is of little importance in fracture repair, and does not require special consideration. At some distance above the line of fracture we do usually find that a thin shell of new compact bone has been, or is being, laid down around the bony shaft. The long centripetal course of the leading vascular channels is usually a striking feature in such specimens. It is as if the bony new growth were forced outwards against the periosteal membrane, stretching its vessels as it advances. And if we are to speak of such bone as medullary in origin, such language would be more than figuratively correct.

*Tendon and Ligament.*—In most fractures some tendinous or ligamentous tissue is implicated, and is frequently concerned in the process of repair. The tendons proper require less consideration than the muscular insertions. Ligamentous tissues everywhere, interosseous, capsular, &c., when involved in a fracture, respond readily to the ossific stimulus.

It might almost be said that tendons or ligaments are endowed with an excessive or morbid tendency to become ossified. Even where there may have been no fracture of bone, only rupture of tendon, instances of development of new bone in the lacerated tissue are by no means rare. The best known examples of this occur after rupture of the tendon of the adductor longus muscle of the thigh in horse exercise. In the interosseous ligaments of the arm and the leg, an excessive development of bone is liable to take place after fracture or other injury or inflammatory disease. Bony ridges or spicules are sometimes found springing from the bone, and replacing intermuscular septa, and probably owned similar causes. And cases have been recorded in which, without any external traumatic cause, there seems to be a general tendency to the growth of bone in the ligamentous tissues throughout the body.<sup>1</sup> It is evident, therefore, that fibrous tissue is a medium favourable for the development of new

<sup>1</sup> Cf. Hancock, *Lancet*, 1866, vols. i. ii.; Bennet, *Dub. Journ. Med. Sc.* 1872, vol. i.-iv. p. 510; Hamilton, *loc. cit.* p. 508; Henry, *Phil. Trans. Lond.* 1759, vol. li. p. 89.

bone; and in a less direct, but more important manner, it may be the means of carrying the ossific stimulus outwards into other tissues which might otherwise remain passive in fracture repair.

In the embryo the junction of tendon with bone is effected by means of a tissue which resembles true cartilage, and which has been spoken of as tendon cartilage. The insertion of the tendo Achillis of the frog will give good examples of this tissue.<sup>1</sup> In the human adult the plump round cells of the tendon cartilage are replaced by the flattened obscure cells of true tendon, and there is nothing left to indicate that the tendinous insertion had differed in any way from the rest of the tendon. But the advent of the inflammatory stimulus at once resuscitates the embryonic memory, and demonstrates the true nature of the tissue. There is not only a reversion to the embryonic cartilaginous structure, but there is superadded to this an influence which the cartilage never succumbed to before—that of calcification and ossification. And not only does the immediate insertion become calcified like ordinary cartilage, but the tendon proper at some distance from the bone may be similarly infected and transformed.

Having taken for granted the ordinary descriptions of ossification of tendons in birds,<sup>2</sup> I was not prepared for the thoroughness of the process in fractures in man. Suspecting that the process of so-called tendinous ossification had not been pursued to its finality, I secured some tendons of old turkeys and examined them carefully. This investigation proved to me that the ordinary descriptions of tendon ossification in birds were not complete; that this so-called ossification is only the preparatory stage of calcification; and that, in old birds and in special positions, we may find a true ossification following up and replacing the calcifying process (fig. 5, Pl. VII.). It happens that calcareous infiltration in birds' tendons begins early and lasts long; and its successor, true ossification, comes on late, after the period that birds most convenient for study are usually killed. So the process may have been overlooked.

It will be convenient shortly to describe the process of ossification of tendons in birds. In a longitudinal section of one of the long tendons the proximity of the calcifying process

<sup>1</sup> Stricker's "Histology," *New Syd. Soc. Trans.* vol. i. p. 107.

<sup>2</sup> Ranvier, *Traité Technique d'Histologie*, p. 455 et seq.

is indicated by the swollen and prominent aspect of the small, flat, almost invisible tendon cells that lie in rows between the bundles of fibre. They lie in straight parallel rows and gradually increase in breadth, but are always longer than broad. Calcareous salts are thrown down in the fibrinous matrix between these cell rows, and ultimately between the individual cells. The calcareous material is very faintly granular, and is in a state of very fine subdivision. The tendon now loses its glassy striated appearance, and ceases to polarise light. The cells, still continuing in rows, increase slightly in size, and send minute processes into the calcified tendon matrix. This is still pure calcification, only it is far more permanent than the like process anywhere else. The great length (comparatively speaking) of the tendons, their compact sparsely vascularised structure, and the absence of overlying muscle on the tarso-metatarsal bones of birds, may help to account for this.

For a considerable distance down the tendon no new appearances may be visible; but in old birds in the middle of the tendon examples of true ossification replacing the calcified tissue may be met with. Fig. 5 is a drawing of such a condition. The tendon has been cut transversely. At the periphery the fibrous sheath is seen surrounding the calcified bundles and sending septa down between them. The tendinous bundles of bony density are seen in transverse section imperfectly isolated by deeply stained cells, which in this aspect appear tailed and branched. These have been called bone corpuscles; and if the tendon in this condition is called bone, we need not object to the term. Lower down we meet with a dark zone of rough granular material which is the calcified tendon tissue disfracted and broken up preparatory to being transformed by osteoblasts into true bone. Below this we meet with the typical lamellar bone with its lacunæ, canaliculi, and Haversian channels all complete.

Longitudinal sections show the same process going on, and in addition gives us good views of the part which vessels play in the process. The vessels of ordinary calcified tendon observe, in the main, a longitudinal course; but channels of considerable size run inwards from the periphery. A deeply stained band of true bone, less granular and opaque than the calcified tendon

tissue, can often be seen surrounding such a vascular channel, in specimens where ossification properly so-called has not appeared. But true ossification is simply an extension of this perivascular growth; more thorough in its preparatory tissue disintegration; more extended in its area; and accompanied with, we might say caused by, a great increase in the number of vessels. True osteoblasts may be seen; and though anything like a specialised marrow cavity is never present, yet spaces of considerable magnitude, containing true myeloid tissue, are frequently met with. But the bone is always very compact. Altogether, with the exception that we have no primary medullary spaces, and that the process of destruction of the calcified matrix is altogether less complete and elaborate, we have here a close resemblance in all essential points to the ordinary forms of ossification in cartilage. The prime distinction, however, relates to the duration of the two tissues. If we regard the life of the bird, calcification is the normal and permanent process; ossification comes in towards the end of the life it is permitted to lead, and may be regarded as unimportant, almost fortuitous. But still it comes in to remind us once more of the completeness and harmony of allied processes throughout the animal kingdom.

This normal ossification of tendons in birds is imitated with considerable exactitude in the inflammatory osteogenesis which lays hold of tendons and ligaments implicated in fractures in man. The similarity to intracartilaginous ossification is more close in the latter even than in the former case. The steps of the process may be followed in figs. 6, 5, and 10. The flattened cells, which in their normal state can scarcely be detected as they lie between the tendinous bundles, start forth from their concealment and appear in closely-set regular rows. They go on increasing in size, becoming oval or even round, and ultimately appearing surrounded by a distinct cell wall and a varying amount of clear pellucid cell contents. The intervening fibrous bundles swell, and become more wavy in texture and more vague in outline (fig. 5). At this stage the structure is very similar to normal temporary cartilage, except that the cells are always in rows, and that the matrix is obscurely striated in lines parallel with these rows. The tissue is now ready for

calcification. As in ordinary calcification, the nodular salts are laid down primarily in the areas of matrix most distant from the cells, and gradually encroaches on the cell capsule. But the calcareous infiltration need not take place in a direction identical with the cell rows. The "piling" of cells in rows is here determined by their normal arrangement between the bundles, and not by any developmental preparation for calcification as in health (fig. 10). Frequently, as we should have expected, the invasion with calcareous material takes place from the side of the bundles, and not along their course.

After a longer or shorter area of calcification, we meet with attempts at vacuolation which correspond to the primary medullary spaces in the normal process. Thereafter may be found examples of the development of true bone by osteoblasts in orthodox manner, and the history of the process is then completed.

But, as a matter of fact, it is not always that the process reaches the stage of true ossification. The advent of the young medullary tissue may cause complete absorption without the formation of any true bone. And it must not be supposed that the changes can be seen taking place evenly or on one level; irregular spicules run in varying directions and at different rates through the parent tissue, and it is only by comparing numerous sections at various levels that the course of events can be followed.

No more beautiful example could be cited of the closeness with which, under novel conditions, a tissue remains true to traditions of its descent than the behaviour of tendinous structures in the neighbourhood of fractures. Dr. Creighton has shown (in a previous number of this *Journal*) how tendon cells exposed to the influence of granulation tissue may become assimilated thereto both in shape and in function. Here we have a change more far-reaching in variety and extent; though perhaps less so in real nature. In fractures the reversion to embryonic form ceases at the stage of fibro-cartilage; having got so far, the ossific stimulus lays hold of it and transforms it in its own direction for its own purposes.

In illustration of this process of tendon ossification, I cannot refrain from alluding to a closely allied one which I found going on in the periosteum overlying two specimens of carious bone. On the anterior aspect of the great trochanter of a femur excised

for hip-joint disease there existed a few hard nodules, the largest about the size and shape of a split pea, rising abruptly from the surrounding bone, and covered with what appeared to be periosteum. On making sections of these I found that they consisted of an outer shell, which was simply a zone of calcifying cartilage, and contents composed of areolar bone and inflamed medullary tissue. Very similar appearances were subsequently found on the under surface of a carious os calcis. The inflamed medullary tissue had burst the compact shell, elevating a blister of periosteum; a protective periostitis was set up, followed by attempts at calcification. The interest was in the fact that the development of new bone was not such as we find subsequently to periosteal inflammation (and such as existed in other parts of these bones), but apparently through the medium of temporary cartilage. The bone had chanced to give way under a ligamentous insertion, the tendon became embryonic and cartilaginous, and behaved under the ossific stimulus as cartilage usually does. No pure periosteal tissue, except, perhaps, under the influence of a very near and very powerful infection, could exhibit such changes.

Such is the nature of the typical transformations of ligamentous tissues concerned in fracture repair. But the other fibrous or cellular tissues abounding in the neighbourhood exhibit similar or allied changes. Sometimes, as in fig. 7, we see a decided change to the cartilaginous form. Fig. 9 shows a mixture of the fibrous and cartilaginous forms, only the cartilaginous tissue is breaking up before it has been calcified. In fig. 13, again, we have a good specimen of calcifying cellulitis abutting on an area of cartilaginous structure. It is curious that in both these specimens the cellular tissue should have become impregnated with calcareous material before the cartilaginous; and this is no uncommon occurrence. Transitional appearances in great variety, many of them of great beauty and interest, but too numerous to be particularly specified, are met with in every specimen of fracture repair.

I believe that the ligamentous tissues and their congeners are of prime importance in fracture repair. They stand pre-eminent in their capacity for becoming ossified. Their wide distribution enables them to carry the ossific infection far out

into the surrounding transformed and neoplastic tissues. If we include ordinary cellular or areolar tissues—which we may with justice do—and remember how readily its meshes become charged with the products of inflammation, we cannot fail to put a high value on this variety of tissue as an element in callus growth.

*Fibro-cartilage.*—Under ordinary circumstances this tissue does not play an important part in the repair of fractures. I have had opportunities of studying its behaviour only in three of these cases, viz., 4, 7, and 9—one of the neck of the femur and two of the vertebrae.

In the femoral fracture a bridge of white fibro-cartilage united the margin of the articular cartilage above with the perichondrium of the neck, and this was infiltrated with bone salts, except where it was in immediate contact with the bone, that is, where movement was most felt. The tissue itself had undergone little change. Irresponsive to the inflammatory stimulus, and being a ready-made matrix for calcareous invasion, this was just as we might have expected.

Very slight changes were found in the intervertebral cartilages. Here the calcareous material was very dense, and the tissue was very hard, clear, and glistening. As a result probably of the fine subdivision of the salts, this form of calcification would seem to be very permanent, for it existed in the case of eleven months standing (fig. 12). In both the cases of fractured vertebra the calcified fibro-cartilage took an important share in consolidating the union. At parts where the overlying ligaments came in contact with the intervertebral fibro-cartilage, the different behaviour of the two tissues under the ossific infection stood out in striking contrast. In the intervertebral disks, the calcifying process showed a distinct preference for the more purely fibrous portions of the tissue; the central portions were calcified only slightly at their attachment to the bone.

Indications of similar changes were visible in the fibro-cartilage forming the grooves for the tendons of the peronei on a fractured fibula. Slight participation in the inflammatory change, and a passive infiltration with bone salts, seems to be the ordinary behaviour of fibro-cartilage involved in fracture. This is in accord with what we should expect from the normal histology of the tissue.

*Medulla.*—The behaviour of medulla where it makes an exit through vascular foramina and commingles with the exudative material thrown out under a stripped periosteum has already been considered. Yellow marrow can have an influence on fracture repair only by virtue of its sparse connective tissue and small round cells; and this influence, from the very nature of the tissue, must be small. But more is to be expected from the highly organised and sensitive pink marrow which fills up the spaces in cancellous bone.

The existence of living bone may be described as a sort of equipoise between the contemporaneous processes of absorption and deposition. An increment of nutrition begotten of inflammation may, according to its amount, result in a simple addition of new bony tissue or in an absorption of old. Most frequently it starts with excess of absorption, and goes on to increased deposition. In the end the proper balance is recovered by a absorption of the neoplastic bony growth.

In every case the amount of primary rarefaction will vary with the degree of inflammation and the reactive power of the tissues; and the oncome of sclerosis will depend on the rapidity with which the inflammatory excess of nutrition fades away. Though, at the time of examination of any given instance, the naked eye might find very little evidence of sclerosis or rarefaction; yet the microscope seldom fails to reveal evidences of previous bony new growth and absorption. This may be concluded when we find numerous deeply, stained laminæ overlying the old bony trabeculæ, and filling up jagged hollows caused by the primary absorption; and when also we find that these new laminæ themselves are undergoing absorption with more rapidity than is observed in the normal process during health.

Here we are brought face to face with the harassed question as to the part which the bone corpuscles play in the disintegration of bone. The more I see of the process of absorption of bone, the more I am convinced of the comparatively insignificant part which the bone corpuscles play therein. In the most varied forms of bony absorption I have never met with convincing proof that the bone cells take any active or initiatory function. Conclusions drawn from the appearance of lacunar hollows representing the disintegration of corpuscular areas of



nutrition are not wholly to be trusted. These hollows are just as likely to have been formed by a passive disintegration along the primary lines of construction as by an active absorption by a bone cell extending as far as its limits of vital activity can go. The best specimens of lacunæ which I have seen were derived from the stump of a femur which protruded after amputation, and which was undergoing absorption by healthy granulations. On comparing the leading positions and shapes of these hollows, it appeared that they exhibited a decided preference for certain positions among the Haversian systems, and in these selected spots observed a peculiarity of shape which suggested a different mode of origin. Such positions were where three or more ossicles meet, that is, where the outer lamellæ of several Haversian systems abut on each other. At such points there is a packing of bone which differs in several respects from true lamellar bone. It is always irregular, is not disposed in lamellæ, and, in bones not too old, frequently retains a remnant of the originally calcified framework of the primary medullary spaces. The first deposit of bone is in globes filling up those festooned hollows in the calcified framework, and a considerable amount of it is left behind among the ossicles far removed from vascular influence and subject to little change. The process of pulling down proceeds on the lines of the building up. These calcified globes being removed, rounded hollows are left; the cell support being broken down, the cell is set free. Over and over again the same appearances repeated themselves, viz., the three or four sides of a large absorption cavity composed of lamellæ becoming disfracted in layers; and the angles, which represented also the points where several ossicles joined dug out into several lacunæ filled with granular detritus and active marrow cells. Regarding such instances as crucial, and extending our generalisation from these to less striking examples, we are driven to the conclusion that the bone corpuscles take little share in the process of bony disintegration.

The function of giant cells in absorption is still a matter of some obscurity. I have elsewhere expressed an opinion that their function and purpose have been exaggerated in one form of bony absorption—the carious; and further experience confirms me in the belief that they have generally been made too much

of. There is no doubt that they are found in greatest abundance where bony absorption is going on actively, and in positions which strongly suggest that they take an active part therein. But constant concomitance need not imply a relation of cause and effect. *A priori* it would seem surprising that a large unwieldy mass of granular protoplasm with a few imprisoned nuclei should be chosen for the delicate and difficult process of bony absorption. And, in the face of the fact that by far the greatest amount of bony disintegration takes place in the presence of small round succulent cells with abundant vascular supply, we must admit that giant cells are at least unnecessary to absorption. Everywhere else in the economy special nutritive activity is attended with increase in the number and diminution in the size of the engaged cells. Why should bone be the only exception? I would, therefore, conclude that the giant cells in bony absorption are effects rather than causes;—masses of granular protoplasm which have remained coherent, partly because they happen to have the protection of a bony hollow, partly because they have not been brought under the disintegrating influence of abundant vascular supply.

Whatever may be the part played by the individual tissue elements, there need be no dispute as to the aggregate result. When the pink marrow has responded to the inflammatory stimulus, and become fully vascularised, it causes bony rarefaction to begin with. But alongside of this a development of new bone by the marrow tissue proper is begun on the surface of the old bone, and this new growth is carried out for varying distances, to meet the periosteal callus material. This increment of new bone is usually confined at first to the periphery of the compact bone; over the fractured ends new bone does not appear till a period considerably later. The delay in the latter case is caused, as has been already indicated, by the presence of a greater or less amount of the sluggish clot tissue entangled in the jagged fragments.

So far as I have seen, medullary tissue contributes to fracture repair only by the direct development of true bone. As a connective tissue we might have expected that it would sometimes undergo changes similar to its congeners and become calcified; but I have never found evidence that it does so. This may be

explained by the fact that an inflammation in marrow runs rather to cellular hyperplasia than to serous exudation, and an important desideratum in a tissue of calcification is that it shall be provided with an abundance of extra-cellular matrix.

By causing rarefaction of the surface of compact bone in the neighbourhood of a fracture, and by sending out new bony material into the overlying callus tissue, the marrow may be said to take the chief share in welding together the parts which are to form the permanent bond of union.

The ordinary calcified callus material, by its very nature unstable, is soon completely removed, perhaps never to be replaced by true bone, but the bone that is produced by marrow is perfect from the beginning, and need never be absorbed. The slow restitution of the balance between bony growth and repair, and the general resumption of their proper functions by the injured marrow and periosteum, bring about a removal of surplus bone wherever it may be, and a deposit of new bone wherever it may be required. The rounding off of protruding points, and the restitution of the medullary cavity, processes which may take years to complete, now conclude the cycle of fracture repair.

*Callus Cartilage.*—Billroth,<sup>1</sup> speaking of the appearances met with in a fracture in the rabbit of ten to twelve days' standing, thus refers to callus cartilage:—"The spindle-shaped swelling of the soft parts has, for the most part, the appearance and consistence of cartilage, and agrees therewith microscopically; in the medullary canal also we find fresh formation of cartilage in the vicinity of the fracture. The broken bone lies in this cartilage, just as if the two fragments had been dipped into sealing-wax and stuck together." If it were necessary specially to emphasise the fact that the repair of fractures in man is widely different from the same process among the lower animals, such a description as the above might be appealed to. Except perhaps in young children, we see nothing approaching to this abundant formation of the so-called callus cartilage in man.

In the specimens which I have examined the part played by callus cartilage in repair was almost ridiculously small. In fact, judging from the above, I should scarcely have postulated the existence of such a tissue as callus cartilage at all. I know of no

<sup>1</sup> "Surgical Pathology," *New Syd. Soc. Trans.* vol. i. p. 247.

new tissue resembling cartilage that cannot be referred to a metamorphosis of old tissue. The most perfect mimicry of cartilaginous tissue is provided by transformed fibrous and ligamentous structures. Coming after this, the œdematous out-lying areolar tissues with sparse fibres and large round cells lying in a glassy homogeneous matrix, often have a close resemblance to true hyaline cartilage. Even clot tissue may, as we have seen, be transformed by a process of infection carried bodily through it into something very like cartilage; and other tissues in various degrees carry out the resemblance.

But none of these are true cartilage. They are old tissues transformed into a condition which we know to be histologically suitable for infiltration with calcareous salts, and nothing more. Where these old tissues do not readily undergo inflammatory changes in this direction, the calcifying influence still does its best, and lays hold of them also. This influence does not overlook those sluggish tissues and have recourse only to the specially prepared callus cartilage. Doubtless if the nidus for bony new growth in man were callus cartilage alone, the repair of fractures would be a more rapid and perfect proceeding altogether. But tissues thirty or forty years old in the human adult are not so plastic as in one of the lower animals, which will probably have reached only its fourth or fifth year. An ideally perfect nidus for bony development in human fractures not being forthcoming, the best is made of an imperfect transformation of the old.

I do not argue that this callus cartilage in the lower animals is not a new growth specially provided for the purposes of fracture repair. All I would maintain is that I have seen nothing to show that it is such in man. In fact, had our investigations been confined to human fractures, I doubt if we should ever have heard the term "callus cartilage" at all. At all events we should never have credited the small tracts and areas of cartilaginous-looking tissue which run ahead of a line of calcification or traverse the mass of inflammatory embryonic tissue with being the only, or even the chief medium in which the uniting bone is to be developed.

For the whole process of fracture repair we might give a generalised summary as follows:—

A plastic parent tissue is provided partly by the swollen and inflamed pre-existing tissues, partly by organised inflammatory neoplasm. The ossific stimulus lays hold of this parent tissue and infects it with the first change towards ossification, viz., calcification. The calcifying process proceeds along numerous irregularly disposed lines through the parent tissue modifying it finally before it completely invades it. This modification of matrix ahead of the line of calcification consists chiefly of a swelling of the intercellular substance bringing about an occasional resemblance to ordinary hyaline cartilage. When the process is completed by the formation of an areolar calcified tissue containing variously modified embryonic substance in its meshes, we have the great mass of so-called bony callus. This callus material soon begins to undergo the further changes towards true ossification; namely, absorption of the calcified substance and development on these calcified lines of true lamellar bone by osteoblasts derived from outgrowing marrow or cambium layer of periosteum.

But, as a matter of fact, except in special positions, the completion of the ossific process is rarely reached. The physiological completion in the development of true lamellar bone is seen in the permanent uniting medium between the fractured ends.

A summarised statement of the parts played by the individual tissues concerned may be given as follows:—

*Blood Clot.*—The greater part of the effused blood is absorbed and disappears. Some amount of blood clot usually remains in certain positions to become organized. After organisation, more or less perfect, it may either directly become a parent tissue for bony growth, or undergo further transformation towards this end. Blood clot organises by preference in the exposed medullary canals and over the jagged ends of the broken bones. Where it forms the chief nidus for ossification, bony union is longest delayed.

*Periosteum.*—Periosteum contributes to fracture repair rather by virtue of its position as a fibrous tissue than through its ordinary function as a bone producer. Periosteal callus is rather a calcifying cellulitis than an ossifying periostitis. Periosteum does not undergo very much change preparatory to calcification. The calcified periosteal trabeculae are the least

perfectly developed and the most unstable of all the areolar bony callus material. Shreds of periosteal fibre traversing embryonic inflammatory tissue contribute materially to the rapidity with which calcification is carried out.

*Tendon and Ligament.*—Where present these tissues play an important part in the ossifying process. Sometimes undergoing calcification without exhibiting material change of structure, they frequently, however, are subjected to preparatory modifications which assimilate them in appearance to true cartilage, and thereafter behave in ossification as ordinary cartilage does. The presence of an abundance of ligamentous tissue should warn us of a possible overgrowth of uniting bone.

*Fibro-cartilage.*—White fibro-cartilage may become calcified without undergoing preliminary structural changes. The calcareous deposit is finely divided and dense; and the advent of the absorptive process is long delayed. As a temporary bond of union it is, when once formed, the strongest of all.

New inflammatory tissue contributes to bony growth after it has been in varying degrees assimilated to the formed tissues in its neighbourhood. Occasionally it assumes a resemblance in structure to hyaline cartilage.

This must conclude for the present our study of fracture repair. In a future paper I hope to extend these observations by clinical applications. If for any given case of fracture, by forming an estimate of the amount of injury present, and by bearing in mind the nature of the tissues involved and the potentiality of these to serve as bases for bony growth, we should be able to observe special precautions towards the promotion of union, or to give a more certain prognosis as to the period when union will be complete, we should have made a step forward in our treatment of this large class of surgical injuries. In most cases I believe this can be done. Some fractures I believe to be systematically overtreated; others are not kept for a sufficient time at rest. Occasionally, where there is a superabundance of callus matrix, our determination to secure perfect union runs to the extreme of bony overgrowth and perhaps ankylosis. And, on the other hand, are we not too prone to act upon the experience that certain fractures rarely

unite by treating them as if union never were possible? To the solution of these and such problems our study of the process of fracture repair ought to be directed.

## EXPLANATION OF PLATES VI AND VII.

**PLATE VI.**—Figs. 1, 2, 4, and 5. Representing the process of alveolation of blood-clot. Fig. 1 ( $\times 300$ ) shows four epithelial cells embedded among the blood cells. Fig. 2 ( $\times 300$ ) represents the formation of a hollow apparently by means of those large cells. Fig. 3. The commonest forms of trabeculae in clot, the largest and smallest being here in contact. Fig. 4 ( $\times 300$ ). A more advanced stage of the same. Fig. 5 ( $\times 300$ ). A portion of the wall of a large cavity which was surrounded by thick trabeculae, and lined with large flattened cells. Fig. 6 ( $\times 500$ ). Specimens of large yellow granular cells (? hæmatoclasts) distributed through the clot tissue. In some of them are red blood discs. Fig. 7 ( $\times 300$ ). Represents a transformation of blood clot into a structure resembling hyaline cartilage. The gradations are drawn only at one side. Fig. 8 ( $\times 300$ ). Section through fractured articular cartilage with adherent blood clot. Fig. 9 ( $\times 300$ ). A young vascular area in clot tissue. Two tubes are seen in longitudinal, one in transverse section. Figs. 10 and 11 ( $\times 800$ ). Longitudinal and transverse views of the earliest stages of vascular growth. Fig. 12 ( $\times 300$ ). Young vessels in inflammatory callus matrix. Figs. 13 and 14 ( $\times 300$ ). Familiar forms of embryonic tissue, in which the chief amount of bony callus is developed. Fig. 15 ( $\times 300$ ). Changes in clot adherent to a spicule of bone. Fig. 16 ( $\times 300$ ). Calcification of matrix around fat cells.

**PLATE VII.**—Fig. 1 ( $\times 100$ ). Areolar calcification of periosteal fibre and growth of true bone by periosteal osteoblasts going on side by side. Fig. 2 ( $\times 350$ ). Transverse section through the tip of a calcifying spicule developing in periosteal fibre. Dense calcified area containing three lacunae is surrounded by a deeply-stained zone of osteogenetic substance. Fig. 3 ( $\times 150$ ). A longitudinal section of a similar spicule. Fig. 4 ( $\times 300$ ). Absorption of areolar calcified callus. (The shading is done too smoothly and regularly.) Fig. 5 ( $\times 100$ ). Transverse section of portion of a turkey's tendon, showing calcification at the periphery (top of drawing), and true ossification in the centre (bottom). Figs. 6, 8, and 10 ( $\times 200$ ). Illustrating the steps in calcification and ossification of tendinous structures in fracture. Fig. 7 ( $\times 300$ ). Transformation of ligamentous tissue into one resembling cartilage, with incipient calcification at bottom. Figs. 9 and 13 ( $\times 300$ ). Transverse and longitudinal sections of calcified fibrous tissue, each with portions of a substance resembling cartilage. Fig. 11 ( $\times 300$ ). Calcified "callus cartilage" on the top of a spicule of bone. Fig. 12 ( $\times 150$ ). Calcified fibro-cartilage of great density, showing little structural change, and becoming absorbed on one side.

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COLLOID DEGENERATION OF THE NON-CYSTIC  
OVARY, WITH ASSOCIATED VASCULAR CHANGES.<sup>1</sup>

By HENRY S. GABBETT, M.B., M.R.C.P., *Medical Registrar to the London Hospital, Assistant Physician to the Royal Chest Hospital, Pathologist to the Hospital for Women.* (PLATE VIII.)

IN a considerable number of ovaries, especially the ovaries of elderly women, the following appearances may be seen:—Thin sections examined with the naked eye by transmitted light reveal the presence of more or less transparent spots of various shapes and sizes. Under the microscope these spots are found to be for the most part composed of a homogeneous material, semi-fluid or gelatinous: the smaller traversed in all directions by trabeculae of the stroma, the larger clear in the centre, but showing stromal ingrowths at the circumference. The material contained in these spaces is unaffected by acetic acid; it is readily hardened by alcohol and other agents, which at the same time no further alter its appearance than by rendering it whitish and opaque when seen by reflected light, and by producing a certain amount of shrinking and cleavage. Thus satisfactory sections may be made from a hardened ovary and stained as desired. Hæmatoxylin gives good results: when a double staining is produced with hæmatoxylin and eosin, a very beautiful preparation may be obtained, the material of the softened patches being deeply coloured by the eosin, while the stroma and its prolongations take the logwood more especially. Carmine stains the new substance; picric acid does not affect it. Iodine produces no coloration.

Stained sections, mounted in glycerine, dammar, or Canada balsam, show that the very smallest patches, which are of microscopic dimensions, not exceeding the size of two or three

<sup>1</sup> This paper was written before the publication of the *Journal of Anatomy and Physiology* for July 1881, and therefore contains no reference to an article by Dr Vincent Harris and Mr Alban Doran on the "Ovary in Incipient Cystic Disease. But I have added a note to explain the reasons why I differ from their conclusions in a part of their article.



undeveloped ovisacs, are usually irregularly oval, the long axis being determined by the general course of the tissue of the stroma. They are minutely subdivided by fine trabeculae, which have all the appearance of being teased out, so to speak, by the accumulation of some substance within their meshes. These stromal trabeculae consist of fibres and cells apparently quite unaltered, and in favourable specimens minute blood-vessels may be seen in them. The intruding material is structureless, resembling ground glass in appearance, and contains no trace of cells or nuclei. The spaces are bounded by the normal stroma, which shows no sign of condensation or any other change in their neighbourhood. There is no increased vascularity about the patches. The larger spaces can be shown in many cases to result from the fusion of several of the smaller: they are in all important respects identical with the latter; processes of fibro-cellular tissue run into them, accompanied by vessels, and the contained substance is microscopically similar. In some of the larger patches, however, the central parts show a new process of degeneration: instead of the cloudy material elsewhere observed, the centre is composed of coarsely granular *débris*, often associated with altered blood, the result of old extravasations. Moreover, the trabeculae in these patches are commonly of considerable dimensions, but never completely traverse the spaces. It may be noticed here that in some of these larger degenerate structures, such as can be easily recognised with the naked eye, the boundary line of stroma may seem at first sight, when examined without the microscope, or with a very low magnifying power, to present a very distinct demarcation,—so distinct, in fact, as to resemble a cyst wall. Closer observation, however, will always show this appearance to be deceptive, and to be mainly produced by the shrinking of the new substance under the influence of the hardening agent. In the very largest patches, I have always found processes running inwards from the stroma, and the neighbouring tissue unaltered except by the existence of minute degenerate points such as those described above. There is never a trace of epithelial lining. In the more advanced stages, as in the earlier, there is an entire absence of increased vascularity; but there is a remarkable change in the blood-vessels, which will require a separate description. (In the

figure, *a* represents an early stage of the change, and *b* part of a large space, as seen with No. 7, Hartnack.)

Not unfrequently associated with this change in the ovary is another entirely distinct from it, which may be briefly alluded to here to prevent errors in observation. In some of the transparent spots mentioned at the beginning of this paper, there is no deposition of a new substance, but simply a destructive rarefaction of the stroma, together with the formation of branching cells with long processes; a fine web of delicate filaments is seen in these spaces; some of the filaments are apparently derived from the tissue of the stroma, others are prolonged from the branching cells; within the meshes we find altered blood and large granular bodies with distinct nuclei. The spaces seem to grow rapidly, they contain no vessels which can be recognised, and their centre always breaks down in a short time into a mass of *débris*. I may add that in two or three instances I have found these filamentous patches bordered by stroma, which showed an infiltration of the colloid material; but beyond this apparently accidental association, the two processes seem to be essentially distinct. Whatever the significance of these structures may be, they have no connection with the subject of this paper.

With regard to the conditions under which the colloid change occurs, I can only speak from my own observations, having been unable to discover in any author a minute description of the change in the non-cystic ovary. (1) It is most frequently met with in the atrophied ovaries of women after the menopause. (2) The degeneration is often associated with chronic ovaritis or peri-ovaritis with adhesions. (3) It is occasionally seen in ovaries which have been the subjects of repeated and intense congestions, as indicated by follicular hæmorrhages, &c. (4) Lastly, I have found an early stage of the change in ovaries which in all other respects seemed normal. I have not as yet seen it in any ovary before the age of twenty. The specimens exhibiting the most advanced stage have been from elderly women. The degeneration is also met with in ovaries beginning to be cystic, and in the walls of cystomata; but with these cases I am not at present concerned.

In investigating the origin of the colloid patches, the first

thing requisite is to discover their earliest form. Of course, the comparative size of the spaces as seen in a section is of no value in determining this point, since (1) the apparently small spaces may be in reality only parts of larger ones, and (2) the small may be older than the large, derived from them by a process of contraction. Bearing in mind, however, these and other obvious sources of error, it is easy to satisfy oneself by a series of sections through an affected ovary, that the youngest patches are those described above as of microscopic size, and traversed in all directions by a fine network of delicate stromal trabeculæ. This point having once been settled, it seems evident that the change does not originate in Graafian follicles, or in the foetal glandular tubes of Pflüger and Waldeyer; since in the earliest stage the spaces are minutely subdivided by processes of ovarian tissue, and never show a vestige of lining epithelium. It is equally clear that the change is not an inflammatory one, in the ordinary sense of the term. Everything tends to show that the youngest spaces are produced either by infiltration of a new material, or by localised softenings of the intercellular substance. The latter view is the more probable. That the intercellular substance is the part primarily at all events affected, appears to be shown by the fact that the cells of stroma immediately bordering the spaces exhibit no change. With regard to the precise nature of the degenerative softening, it is hard to offer any definite opinion, the analysis of the substance being attended by considerable difficulties: the term "colloid" is used throughout this paper simply to indicate the formation of a semi-fluid or gelatinous product of degeneration which does not exhibit the mucoid reaction with acetic acid.

The following are briefly my conclusions as to the history of this process in the non-cystic ovary. The microscopic patches (which by the way are usually found in the neighbourhood of Graafian follicles of some size; never, I think in the zone of minute ovisacs near the surface, and never in the tunica albuginea;) grow slowly by the extension of the softening into the surrounding stroma, and by the junction of neighbouring patches. The growth spreads irregularly, so that after a time the space contains not only the minute primary trabeculæ, but also large portions of the stroma which have escaped the change. The

fine trabeculae persist for a considerable time, but at last for the most part disappear, leaving traces in the shape of obscure lines in the colloid mass. The growth of the spaces does not proceed indefinitely, but after a certain period comes to a stand-still; when this occurs, the surrounding stroma appears, as it were, to accommodate itself to the shape of the mass, and the borders assume a smooth, scalloped appearance, resembling in some measure (as already mentioned) a cyst wall. At the same time, it is common to find granular degeneration in the centre of the mass, and some cirrhosis of the ovarian tissue generally. As a rule, no further change occurs, save consecutive atrophy of the organ. Thus, an ovary in the final stage of this metamorphosis is usually small, hard, corrugated on the surface, and consists of a shell or envelope of dense tissue, enclosing a coarse framework which contains the colloid substance, and in the centre a fibrous core supporting the blood-vessels.

Structures similar to, or identical with, those above described, have long been recognised in cystic ovarian tumours. Rindfleisch's description of the appearances found in the wall of a cystoma of his second variety is of course well known. Even such an early writer on ovarian pathology as Kiwisch has given a minute account of a form of alveolar degeneration, which consists in a breaking up of the stroma into closely aggregated alveoli, enclosing a homogeneous substance of varying degrees of consistency. Again, Dr. C. G. Ritchie (*Contributions to Ovarian Physiology and Pathology*, 1865), describes "the variety of tumour in which the substance known as colloid is most frequently found" as originating in "a hypersecretion into the meshes of stroma which enclose the first trace of the Graafian vesicle," which "stromal meshes may break into each other, and form larger or smaller cavities."

It is to be noted that these and other observers refer only to appearances found in cystic tumours of some size, and assign importance to the colloid change as representing an early stage in the growth of the tumour. None of them, so far as I am aware, give us any reason to believe that they had observed the same phenomenon in the non-cystic ovary, or were at all aware of the great frequency of its occurrence. There is an obvious difficulty in accepting any theory of a degenerative process, pure

and simple, producing a cyst greatly exceeding the normal dimensions of the affected organ. It is, however, impossible to prove that the colloid spaces may not under certain conditions acquire a secreting epithelium, and thus in process of time become true retention-cysts. This is undoubtedly Rindfleisch's view of the destiny of the transparent spots which he figures in the wall of a cystoma; and he even seems to represent an epithelial lining in some of the spots still very minute, and still traversed by stromal trabeculæ. This, however, is certainly not what takes place in ovaries such as I have described. My observations lead me to think that colloid softening is one of the commonest accidents which happen to the ovary; that it has no necessary connection with cyst-formation; that it may be found under certain conditions wherever the stroma exists, whether in the organ otherwise healthy, or in the wall of a cystoma; and that, whatever may be its ultimate destiny in the latter case, in the former it simply replaces the normal stroma by a product of degeneration, which, together with the commonly associated cirrhosis, tends to cut short the functional activity of the ovary, and finally to bring about atrophy.

Allusion has been made to a remarkable change in the blood-vessels associated with colloid degeneration of the ovary. The altered vessels are usually in the immediate neighbourhood of colloid patches of some size, and sometimes penetrate into their interior. In the most advanced stage the artery is so changed as to present the appearance of a solid cylinder without any lumen, the transverse section showing as in the fig. at *c*. Three zones are to be observed in these transverse sections; externally a thin layer of unstriped muscle, next a broad band of homogeneous material occasionally containing a few isolated muscle-cells, and in the centre a mass of nuclei heaped up in confusion, which seem to be derived from the endothelial coat. In the less advanced stages a lumen may be detected in the centre of the vessel. The change seems in all cases to have its origin between the muscular coat and the endothelium: the resulting clear material stains very deeply with eosin, so that the diseased vessel presents an appearance which immediately attracts the eye in sections prepared with that agent. The affected vessels are always arteries, from a size just visible to the naked eye

down to that of the smallest arterioles. I have seen nothing to show whether the change commences in the former or the latter class; it is found equally advanced in both in the same specimen. The larger vessels entering at the hilus are never affected. I can offer no suggestion to throw light on the nature of the degeneration; it is certainly not amyloid, and bears no resemblance to the change in chronic Bright's disease; beyond the fact of staining with eosin just mentioned, I have found no characteristic reaction in the altered coat.

This arterial disease is so commonly associated with colloid degeneration, that I cannot remember having met with the latter without some evidence of the former, and the more advanced the colloid change the more the vessels in the neighbourhood seem to be affected. There would seem, therefore, to be some connection between the two phenomena; perhaps the vascular metamorphosis is secondary to the degeneration in the ovarian tissue, perhaps (and this appears more probable) the latter is due to the alteration in blood supply caused by the arterial disease. The connection is, however, by no means a necessary one, since the altered vessels are certainly to be found in ovaries which seem healthy, and in ovarian tumours of different kinds.

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#### DESCRIPTION OF PLATE VIII.

- a, a.* Represent early stages of the change in the stroma.
- b.* Is part of a large colloid mass. Stromal trabeculae of various sizes are seen passing some way into the interior.
- c, c.* Show arteries in an advanced stage of disease.
- d.* A vessel with lumen still distinct.
- e.* Represents part of a follicle of moderate size, containing some coagulated liquor folliculi.

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*Note.*—The authors of the article on the "Ovary in Incipient Cystic Disease" (*Journal of Anatomy and Physiology*, July 1881), put forward the view that one variety of cyst originates in a process of abnormal involution of Graafian follicles, in which there is an active ingrowth from the surrounding stroma, and a long persistence of certain remains of the membrana propria. The description and figures of the bodies resulting from this process, correspond very closely with the appearances which in the foregoing paper I have ascribed to a degeneration of the stroma. I should be more certain of the identity of the phenomena, were it not for the authors' allusions to "opaque tube-like masses" in the spaces, an appearance which, after careful re-examination of my

own sections, I have been unable to detect. Assuming, however, that we have both been observing the same structures, it is clear that our conclusions differ widely. I am not sure that I have quite followed the reasons which have convinced Dr Harris and Mr Doran of the intra-follicular origin of the colloid masses, an origin which they believe to have been overlooked by other observers; nor have I found in their article any reference to, or explanation of, the very minute spaces described above. Again, it is by no means clear to me how a process of "active ingrowth" of stroma into pre-existing spaces can have anything to do with cyst-formation; nor why, if we accept that view, it should not be easy to trace the steps of the process from these cystic bodies up to unmistakeable cysts. Indeed, I have been unable to find in the article any reason for believing that the spaces described are to be regarded as the origin of cysts, further than the fact that the authors have detected them in ovaries which "afforded presumptive evidence that they were in a state of incipient cystic disease"—a fact which cannot be held to prove much if similar structures are often found in ovaries which afford no such evidence. But I shall here confine myself to a very brief statement of the reasons why I cannot believe that the change, as I have observed it, has an intra-follicular origin, or is connected with the development of cystic tumours.

1. If the degeneration took place in Graafian follicles, one would expect to find various stages of the change in the same specimen or in a series of observations on different ovaries, *i.e.*, some follicles retaining an unmistakeable membrana granulosa while exhibiting an early formation of stromal ingrowths, &c., others showing a more advanced stage in the morbid involution, and so on, up to the appearance of *b* in the figure. I have not observed anything of the kind; on the contrary, the smallest, youngest degenerate patches are far smaller than follicles normally possessing a true membrana granulosa; they are far more minutely subdivided by delicate stromal trabeculae than the larger spaces; and they never show a trace of lining membrane or epithelium. That a colloid change does take place in Graafian follicles undergoing retrograde metamorphoses is undoubtedly true; but the resulting appearances do not resemble those above described.

2. The question of the origin of cysts is one of such consequence in ovarian pathology, that there is a natural tendency to assign to all morbid appearances an importance as being somehow connected with cyst development. When my attention was first directed to these colloid patches, I was misled by this tendency, and described them as a form of embryo-cysts.<sup>1</sup> I have since been convinced of my error, for the following reasons:—*first*, because when the change occurs in a cystic ovary, the truly cystic parts bear no resemblance to the parts affected by the colloid degeneration; and *secondly*, because I have found the change most frequently in ovaries which were certainly not cystic, which afforded no reason for believing that they would ever have become so, and which were not the fellows of cystic tumours on the other side.

<sup>1</sup> In a description of the ovaries contributed to a report of a case of Oophorectomy, by Dr Heywood Smith, *British Medical Journal*, July 12, 1879.

ON THE PHALANX MISSING FROM CERTAIN DIGITS  
IN THE MANUS OF *CHIROPTERA*. By G. E.  
DOBSON, M.A., M.B., &c.

IN a paper "On the Ossification of the Terminal Phalanges of the Digits,"<sup>1</sup> the object of which is to demonstrate that ossification of the ungual phalanx commences at its distal extremity instead of at the centre of the shaft, as in the other phalangeal bones, the author remarks, that "the peculiarity described appears to be universal, the same general description applying to any terminal phalanx, whether taken from the manus or pes, and whether belonging to a fully functional or to a mere aborted digit, such as the second and fifth in the manus of the pig. An examination of the growing bone with a view to this point would probably suffice to decide whether the phalanx missing from certain digits in the manus of *Pteropus* and other bats is really the third (as described, see Flower, *Osteology of the Mammalia*, 1876, p. 264), or whether it may not rather be the second."

The question thus raised may, I think, be answered without employing the method of investigation suggested by Mr Dixey.

In all the frugivorous bats (*Megachiroptera*) there are three phalanges in the second digit, but none of the other digits have more than two. Which of the phalanges then is the missing one in these digits? This is easily determined by analogy, if we examine the condition of the corresponding parts in the insectivorous species (*Microchiroptera*).

In *Microchiroptera* a rudimentary cartilaginous third phalanx is present, though hitherto apparently not generally recognised as such, in the three last digits of the manus of most of the species; in many, moreover, as in the species of *Phyllostomidae*, in the genera *Thyroptera*, *Myzopoda*, and *Mystacina*, and in the species *Molossus perotis* and *Vesperugo noctula*,<sup>2</sup> there is a true ossified third phalanx in the third digit, which, as previously

<sup>1</sup> By F. A. Dixey, *Proc. Roy. Soc.* 1880, p. 66.

<sup>2</sup> Professor Flower showed me a skeleton of this species in the collection of the Hunterian Museum, in which an apparently perfectly ossified third phalanx of the middle digit was present.



remarked by me, "corresponds evidently to the cartilaginous extremity of the second phalanx in the other families of *Microchiroptera*, for in some of the larger species of *Molossi* there is an imperfect joint terminating the osseous portion of the second phalanx of this finger.<sup>1</sup>

In the *Vespertilionidæ*, *Emballonuridæ*, and *Nycteridæ*, however, as a general rule, the terminal phalanx of the middle finger is cartilaginous,<sup>2</sup> and, as such, is most developed in the *Vespertilionidæ*<sup>3</sup> (in many species of which there is a distinct but imperfect joint separating it from the second phalanx), and least so in the *Rhinolophidæ* and *Nycteridæ*, where it is as short (*Megaderma*), or nearly as short as the cartilaginous extremities of the fourth and fifth digits; nevertheless, its identity with the cartilaginous third phalanx of the third digit in *Vespertilionidæ* cannot be doubted, and may be easily demonstrated.

In every species of *Chiroptera*, in which the middle digit has a cartilaginous extremity in the full grown animal, there will be found, on making a longitudinal section, a transverse division separating the cartilaginous part from the extremity of the second phalanx. This evidently corresponds to the perfect joint at the end of the second phalanx of the same finger in *Phyllostomidæ*, and the cartilaginous portion cut off by it to the third phalanx. The same condition may be also observed more or less distinctly in the fourth and fifth fingers of many species (for instance, in *Vesperugo noctula* and in *V. serotinus*), whence it follows that the cartilaginous extremities of these digits also are really unossified rudimentary phalanges. Were these, in many cases very short rudimentary phalanges, to suffer still further reduction, and to finally disappear altogether, we should have a condition which is partly typical of *Megachiroptera*, in which there is no trace of a third phalanx in the three last digits. We may, therefore, conclude that the phalanx missing from certain digits in the manus of *Pteropus* and other bats is really the third.

<sup>1</sup> Catalogue of the *Chiroptera* in the collection of the British Museum, Introd. p. x.

<sup>2</sup> In a few species, as, for instance, in *Rhynchonycteris naso*, it appears to be altogether absent.

<sup>3</sup> See Regalia, "Contributo allo Studio dei Chiroterri Italiani," *Rivista Scientifico-Industriale*, Agosto 1878.

## THE HISTOLOGY OF MOLLUSCUM CONTAGIOSUM.<sup>1</sup>

By GEORGE THIN, M.D. (PLATE IX.)

It is somewhat remarkable that the mode of origin of molluscum contagiosum should still form a subject of controversy amongst pathologists. Apparently nothing should be easier than to determine the structures in which these small superficial tumours take their origin. Developing on the surface of the skin, by no means rare in their occurrence, frequently excised and examined, it would seem that their relation to the sebaceous glands, if they have any, is capable of easy determination. A glance, however, at the literature of the subject will convince any one that the question is not so easy of determination, and, for my own part, I can testify that the conclusions which I have arrived at were only reached after a very minute and painstaking examination of a considerable number of tumours.<sup>2</sup>

The molluscum growths which I examined had been excised by Mr. Jonathan Hutchinson from the skin of a young man, over the surface of whose body a considerable number of isolated tumours were scattered. About twenty of the tumours were used in the investigation, and fifteen were divided into complete series of sections, every section being examined. The size of the tumours varied from that of a small pin-head to that of a small pea. The well-known molluscum structure was found in all of them.

In examining the sections, the point to which I specially directed my attention was the relation of the hair follicles and the sebaceous glands to the tumours in their first stage of development. To my surprise, I found only one hair in all the

<sup>1</sup> The preparations on which the views expressed in this paper are based, were demonstrated to the members of the Pathological Society of London during the session 1880-81, and short summaries have appeared in the usual reports of the meetings of the Society and in the "*Transactions*."

<sup>2</sup> Full references to the literature of "Molluscum Contagiosum" will be found in a paper by Dr. Tilbury Fox and Dr. T. Colcott Fox, in vol. xxx. *Trans. Pathological Society*. To these I would add a paper by Virchow, in *Virchow's Archiv*, vol. xxxiii. (1865), a recent paper by M. Renaut in the *Annales de Dermatologie et de Syphiligraphie*, 2nd series, vol. i. No. 3, and a paper by Dr. Sangster in volume lxiii. of the *Medico-Chirurgical Transactions*.

sections. In this instance I found in the centre of a small growth a dilated hair follicle with an attached portion of a sebaceous gland in its normal situation. This appearance I found only once.

A vertical section through a tumour in the early stage of development shows that it is composed of a small number of epidermic processes or growths. Each of these growths begins on the surface of the epidermis, and grows downwards in the interpapillary epithelial cones. As the growth proceeds the superficial and central cells undergo the peculiar transformation characteristic of the so-called molluscum bodies. These new growths, whilst they descend towards the cutis, also develop laterally, the lateral development leading to the fusion in the epidermic layer of the growths which had begun independently and close to each other, the remains of a very thin septum of non-molluscous epidermic cells being sometimes found after the fusion is apparently complete. In the papillary layer of the cutis the molluscum growths develop laterally with greater freedom than in the epidermis, and the cell-masses from the separate growths rapidly approach each other until they are separated only by a thin septum of vascular tissue. The deeper processes also send off secondary offshoots, contributing thus to the growth of the tumour.

These epidermic processes in the cutis assume a somewhat spherical shape, and the outer cells differ chemically and physically from the inner ones. Between the outermost and innermost cells, there are cells which contain granules. These are the appearances which give the epidermic masses superficially a somewhat glandular appearance, and which have, with the position of the tumours, no doubt, suggested the idea that they are hypertrophied sebaceous glands. But these cell-masses have no ducts, and the cells contain no sebum. It would, therefore, be only permissible to consider them as glandular in their origin if they could be traced in their development back to a sebaceous gland as their starting-point. This I have not been able to do, and I have not been able to find in the papers and drawings that have been published in reference to this subject any evidence bearing criticism that it ever has been done.

Towards the centre of the molluscum formation, the super-

ficial cells of the growth break away, leaving a cavity of greater or less depth, and this may take place in preparing specimens if care is not taken. It is the cavity thus formed which has been mistaken for a duct.

In a very early stage of the tumour the molluscum change begins on separate adjacent points of the free surface of the epidermis, the points being situated within a small round area which will eventually form the centre of the tumour. These points are situated in parts which correspond to the centres of the interpapillary cones—the parts of the epidermis which are furthest removed from the vascular tissue. It is difficult to find a preparation which throws light on the first beginning of this molluscum centre, but one of the sections I have examined afforded some instructive appearances.

In this section the centre of the tumour is represented by a deep cavity partly filled with molluscum cells, and the nature of the cavity is shown by one of its walls being formed by the external and internal root sheaths of a hair. It is, in fact, a very much widened hair follicle, from which the shaft of the hair has disappeared. On its side a portion of a sebaceous gland is still present. This portion of gland is taking no part in the molluscum tumour. Small independent centres of molluscum change surround the opening of the affected follicle.

The continuation of the epidermis surrounding the mouth of the follicle with the molluscum cells in the follicle, is evident in this section.

That in the large number of tumours which I examined, every hair and sebaceous gland had disappeared, with this solitary exception, seems to me to be sufficient proof that the development of a molluscum growth is attended at a very early stage with loosening of the hairs, atrophy or obliteration of the sebaceous glands, and changes in the follicle that make the identification of the latter difficult, and in most cases impossible. If the change began on the interfollicular free surface of the epidermis, one would expect to meet with traces of hairs and sebaceous glands more frequently. But if, as in this section, the change begins always in the follicle; leading to early destruction of the hair and gland, the absence of these structures in the tumours becomes intelligible. It is, however, quite possible that a morbid

change may frequently begin in the follicles, and in rarer cases begin in the interfollicular epidermis.

In the affected hair follicle in question, the molluscum cells seemed to be intimately connected with the cells of the internal root sheath. The great distension of the follicle shows that there has been much pressure exercised by the contained molluscum cells, and that there must consequently have been an abundant new formation of them. These cells are probably contagious in their nature, and if they are so the separate independent minute growths which spring up around the central one, and finally join it, are accounted for. Many of the cells which escape from the mouth of the distended follicle must come in contact with the adjacent epidermis, and some of them will adhere to it.

By comparing a series of sections made through one small tumour, it can be shown that the secondary epidermic formations which surround the central larger one never had any connection with a hair follicle or gland.

Virchow, writing in 1865, expressed his belief that the molluscum change begins in the hair follicles. The facts recorded above confirm his statements.

The development of an epidermic cell into a molluscum body has been so well described by previous observers that I have nothing new to offer on this point. As, however, their accounts are not all in accordance with each other, I will give a short description of the transition changes as I have observed them. Of the latest formed epidermic cells of the growth, those on and near the periphery, many show unequivocal signs of nuclear degeneration. Whether all these cells die, or whether some of them undergo the molluscum change I have not determined.

The first certain change is that of the cell becoming filled with minute granules, the nucleus remaining entire and being generally found near the wall of the cell. Clear spaces are seen in this granular substance—so called vacuoles. In succeeding stages the granules become larger and fuse into a homogeneous substance, the nucleus in the meanwhile losing its spherical form and becoming finally lost. In the ultimate stage, this homogeneous substance fills the whole of the cell, which is then known as a molluscum body, but the cell-wall is not involved in the change, and retains its epidermic character. In the final

stages the molluscum substance may fall out and leave behind it the horny capsule in which it was contained.

This newly formed substance stains a reddish brown with eosin, not of so bright a shade as the ordinary normal epidermic horny cell.

The cells of the growth have the ordinary spikes or prickles characteristic of epidermic cells. The prickles can be observed most readily in the intermediate cells between the outermost row and the cells in which the molluscum substance is being deposited.

The appearances which I have described show that molluscum contagiosum is a tumour dependent on the development in the cells of the epidermis of a new substance, of the nature of which we are as yet ignorant. This development begins in the first instance in the hair follicle, between the root sheaths and the shaft of the hair, but soon takes root amongst the cells on the free surface of the epidermis which surrounds the follicle. It is attended with a continuous growth of epidermis downwards into the cutis, the successive layers of newly formed cells becoming successively the seats of the specific change. The development of this epidermic tumour is not attended with destruction of connective tissue like the epidermic growths of cancer, but its action on the vascular tissue is more than can be accounted for by mere pressure. The imperfectly developed fibrous tissue by which the tumour is surrounded testifies to the reaction produced by the influence of an epidermic growth containing elements foreign to the healthy body.

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#### DESCRIPTION OF PLATE IX.

Fig. 1. Section through a small tumour in which molluscum changes were found in a hair follicle. *ep*, Molluscum formation on the free surface of the epidermis near the mouth of the follicle; *s*, sebaceous gland; *i*, internal root-sheath of a hair; *ex*, external root-sheath; *m*, molluscum cells in the cavity of the follicle, from which the shaft has disappeared. (Low power.)

Fig. 2. Section through a tumour in the early stage, showing the formation at separate points in the centres of the interpapillary projections in the molluscum area. *a*, Septum of epidermic cells which

have not undergone the specific change, separating coalescing masses of molluscum cells; *ep*, molluscum cells formed in the centre of the interpapillary epithelium. (Low power.)

Fig. 3. Epithelium cells on the border of the molluscum formation. Magnified 1000 diameters.

Fig. 4. Illustrating the transformation of epithelial cells into molluscum bodies. The cells *a*, *b*, and *c* show the changes in the protoplasm and the disappearance of the nucleus; *d* shows a cell in which the molluscum substance is formed; *e* shows two cells, one consisting only of the empty cell wall, the molluscum substance having fallen out: in the other the molluscum substance is still present within the epidermic sheath; *f* shows a block of molluscum substance which has fallen out of a cell.

CASE OF OBLITERATION OF THE PORTAL VEIN  
(PYLEPHLEBITIS ADHESIVA). By WILLIAM OSLER,  
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McGill University, Montreal.*

THROMBOSIS and suppurative pyelephlebitis are the affections most commonly met with in the portal vein. A few instances of calcification and extreme fibroid thickening of the walls are reported. Organic occlusion, by conversion of the vein into a fibrous cord or mass of connective tissue, is a very rare lesion, as in cases of thrombosis life is usually terminated long before organisation of the clot can take place. The following instance of it presents many interesting features, anatomical and clinical:—

J. C., aged 28, admitted into the General Hospital, under my care, June 17th, 1881, in a condition of extreme exhaustion, consequent upon loss of blood by vomiting. My house-physician, Dr Andrew Henderson, obtained from him the following history:—Has always been strong and healthy; somewhat intemperate, but a steady worker. Has never had syphilis. No constitutional disease in his family. Last September, when engaged in some very hard work, was obliged to give up on account of weakness and dull heavy pain in the upper region of the belly. It was never very localised, and was not aggravated by eating. Patient had to be in bed most of the time, and at about the end of a month had an attack of hæmatemesis, vomiting more than two quarts. Did not leave his bed for some weeks; does not remember whether his legs or abdomen were swollen. Did not go to work until about April, when he got employment in a manufactory as fireman. Latterly, he was put to heavy work, piling bags of sugar, and yesterday (16th) he had to give up owing to feelings of great weakness. This morning he vomited a large quantity of blood, partly fluid, but mixed with clots. When admitted in the evening he was in a state of great exhaustion; surface blanched; pulse very small—135; temperature, 100°; respirations, 20. Shortly after getting to bed he



vomited about four ounces of dark altered blood. A peculiar cadaveric odour was noticed in the breath.

June 18th. At the morning visit patient was examined, and the following condition noted:—He is a large, well-made, muscular man; lies on his left side in a drowsy, semi-conscious state, but can be roused. Face and general surface blanched; no distension of abdomen; superficial veins not visible; no oedema of the feet. Examination of *chest*, negative; *abdomen* flattened; skin of marble whiteness; visible epigastric pulsation; on palpation, marked pulsation in umbilical region, forcible, vertical in direction; no tumour to be felt. On auscultation a remarkable double murmur was heard midway between the navel and tip of ensiform cartilage; to be heard also a little to the right of the middle line, but was very feeble to the left. There was not a cardiac murmur. *Liver*—edge could not be felt; area of dulness much diminished; could scarcely be detected in sternal line; was 3 cm. in mammary, and 4 cm. in axillary lines. *Spleen*—not to be felt below costal border; area of dulness increased, 11 cm. in vertical, 13 cm. in transverse, directions. Patient did not complain of pain during the examination. Bowels have not been moved; a large external pile, filled with coagula, was found on the right margin of anus. Urine clear, and normal.

19th. During the night patient vomited a large quantity of bright, liquid blood, soaking the bed and covering the floor in the neighbourhood. He was found in an apparently dying state, but rallied on the administration of stimulants. The examination at the visit did not elicit any new facts; the epigastric murmur was not so distinct.

20th. No further hæmorrhage, debility extreme, and a tendency to delirium. Slight oedema of feet. The cadaveric odour was very perceptible.

On the 21st and 22d patient remained in a state of profound exhaustion, and there were no additional abdominal symptoms. On the 23d there were repeated syncopal attacks, during one of which he died.

*Autopsy*.—Body well nourished; skin blanched; no distension of abdomen; cutaneous veins not visible; slight oedema of feet.

In abdomen the coils of small intestines were of a very dark

slate colour; peritoneum smooth; colon distended; no exudation; liver and spleen did not appear below the costal border. In thorax a few ounces of serous fluid in each pleura.

*Heart* was flabby and pale; chambers contained small clots; valves were healthy. Aorta normal.

*Lungs* pale, cedematous at bases.

*Spleen* greatly enlarged; weighed 675 grms.; was intimately adherent to the diaphragm and stomach. Capsule was very thick, in places wrinkled, and a firm, semi-cartilaginous plate existed at its convex border. On section organ cut with great resistance, creaking under the knife; the trabeculae thickened, rough, and in spots gritty. Some of the veins were dilated, and contained thrombi. Near the hilus was a wedge-shaped calcified mass, the size of a walnut. The artery was very tortuous, and at the hilus presented a group of small saccular aneurisms, the size of large peas; the coats thickened, partly calcified, and one of them contained an old thrombus, which had softened in the centre.

*Liver* small, and closely united to the diaphragm and to the abdominal wall on the right side. It measured 25 cm. in breadth and 16 cm. from front to back. The shape was retained, but the left lobe was almost completely atrophied, only a small thin tonguelet remaining. The surface was smooth, but towards the right border and behind, many fibrous bands passed between the capsule and the diaphragm. The capsule on the under surface was opaque but smooth. On section, tissue uniform, pale reddish-brown colour; acini distinct, but no perceptible increase in the inter-lobular tissue. The anterior border and the remnant of the left lobe were firmer, and the connective tissue strands between the lobules could be seen. The hepatic veins were of full size. Portal canals not numerous, small; artery and duct distinct (condition of portal vein will be described under venous system).

Microscopic examination showed the liver cells to be somewhat fatty; the connective tissue on the greater portion of the right lobe was not specially increased, but at the anterior border and in the small portion of the left lobe the secreting substance was a good deal atrophied.

*Gall-bladder* contained a quantity of yellow bile. Gall duct normal. Hepatic artery almost double the usual size.

*Stomach* was capacious and contained a quantity of dark liquid mixed with food. Veins beneath the muscular coat could be seen dilated and tortuous. The mucosa was pale, here and there marked with spots of capillary injection. There were two small superficial losses of substance near the cardiac end; the tissue about them was not injected, and their bases were scarcely as deep as the submucosa. About the pylorus the membrane was mammillated, and on section very tough.

*Intestines* contained dark tarry faeces; mucosa pale throughout. Rectum presented a number of enlarged veins just within the sphincter, and the external tumour was found to be a collection of enlarged and thrombosed veins.

*Kidneys* of normal size, pale, a little firm. On section a large quantity of thin watery fluid oozed from the surface. Ureter and bladder normal.

*Venous System.*—On dissecting the gastro-hepatic omentum the portal vein was found to be obliterated from a point 2 cm. beyond its origin, and converted into an irregular, fibrous cord, with ill-defined margins, being matted with the surrounding tissues. In this state it entered the hilus of the organ, and penetrated the main divisions of the portal canals; no trace of the natural appearance of a vessel could be seen. On section the tissue was spongy, not indurated, and somewhat reddish in colour. There were no remnants of a thrombus, nor any cretaceous or calcified portions. The main branches within the organ were also occluded; the connective tissue of Glisson's sheath was abundant, and firmer than normal. Both artery and duct in these parts could be slit open readily. There was not any special contraction about the hilus, and the liver substance in immediate neighbourhood of the portal canals looked normal. The remaining portion of the portal vein and its branches were in the following condition:—Just beyond the junction of the splenic and superior mesenteric was a large saccular dilatation, the size of a walnut, with thickened walls, and here and there a calcified plate beneath the intima. It was in contact with the under surface of the right lobe close to the hilus. *Superior mesenteric* was much dilated; the terminal part, just behind the pancreas, presented several small sacculi, the intima of which contained atheromatous plates. The mesenteric

branches were moderately enlarged. *Splenic vein* admitted the index finger freely, and all its branches were dilated. Several of those on the anterior margin of the spleen were full of firm thrombi. The vasa brevia from the fundus of stomach were dilated, and some of the larger branches contained thrombi. The left gastro-epiploic was almost as large as the splenic, and at the curvature presented several large dilatations, one of which admitted the top of the thumb. The gastric vein emptied into the dilatation just beyond the junction of the superior mesenteric and splenic, and was also enlarged. The walls of all of these vessels were thickened, the intima a little roughened, and in spots calcified. The *inferior mesenteric* was moderately enlarged; the hæmorrhoidal branches were distended. *Inferior cava* normal; openings of hepatic veins presented nothing unusual. Among its branches the lumbar appeared large, particularly one passing by the side of the third lumbar vertebra. To the left of the aorta was a large vein nearly equal in size to the inferior cava; it terminated below by two branches, one of which passed over and joined the junction of the external and internal iliacs on the right side; the other joined the common iliac of the left side. The appearance of the parts after dissection suggested a double inferior cava. Unfortunately the liver, together with stomach, pancreas, and spleen had been removed before this condition was detected, so that the upper termination of this vessel could not be made out. Two large branches joined it above, but their connection could not be traced. The *iliacs* were large; many of the branches of the internal divisions were thrombosed. The *diaphragmatic* veins formed a close plexus, particularly in the œsophageal region, which united with the veins of the coronary and lateral ligaments of the liver. The *œsophageal* veins were numerous and large, and formed a rich network about the cardia. The veins in the suspensory or round ligaments were not dilated. In the thorax the lower intercostal veins were very large, particularly one running along the lower margin of 10th rib. The *vena azygos major* almost equalled the inferior cava in width, and admitted the index finger easily; the azygos minor was also of large size.

*Remarks*—Such a case as the above presents many points of

interest. Sudden and violent hæmatemesis in a young man; no ascites, no enlarged abdominal veins, small liver, large spleen, and a localised murmur in the epigastric region—the diagnosis was not easy, but it lay, I thought, between cirrhosis of the liver, splenic anæmia, and an aneurism. For cirrhosis as a cause of the hæmatemesis were—history of spirit-drinking, diminished volume of liver, increase in size of spleen, and the existence of piles; against were—the age of patient, and the absence of many important signs, as gastric or intestinal catarrh, ascites, and enlarged veins. The well-nourished state of the man, the moderate enlargement of the spleen, and normal aspect of the blood, were opposed to the idea that the primary trouble was splenic. The suddenness of the attack, the brightness of the vomited blood, together with the existence of a localised murmur in the epigastric region—the origin of which remains obscure—suggested the occurrence of a small aneurism, either of the aorta or one of the branches of the coeliac axis; but the hæmorrhage in September, the absence of any pulsating tumour, and the state of the liver and spleen, seemed fatal to this view. Altogether, in spite of the absence of many of the important symptoms usually present, the most satisfactory diagnosis appeared to be portal obstruction from cirrhosis. The evident reduction in the volume of the liver was strongly in favour of this view, and as I had also met with several instances in which severe hæmorrhage had been the initial symptom, I was the more inclined to regard it as an anomalous case of this nature. The history of a hæmorrhage in September, followed by an obscure illness of some months' duration, pointed to a chronic malady.

The history of the case offers no clue to the cause of the obliteration of the vein, but we may suppose it to have taken place in the way in which veins usually become occluded, viz., by the formation of a thrombus which organised, and was ultimately converted into a fibroid cord. Apart from marasmatic conditions, in which portal thrombosis occasionally occurs, coagulation of blood in the portal vein is met with—1st, as an effect of compression, as in cirrhosis, in which the pressure is exercised within the liver, or in tumours in the neighbourhood of the hilus, which compress the main trunk in the gastro-hepatic omentum; 2d, by extension of inflammation from the

bile passages, as in cholangitis from obstruction by gall-stones ; and 3d, by the extension of inflammation or transference of emboli from suppurating or ulcerative foci in the territory of the portal vessels, but in these instances the thrombi which form rapidly soften, and suppurative pylephlebitis is the result. I have met with cases of pylethrombosis from the above causes, but, so far as can be ascertained, none of them have prevailed in this case. The only possible source which is suggested by the *post-mortem* is the cretaceous area in the spleen, representing the final stage of a small abscess or infarct, which, when in an active state, might have induced, by direct extension or embolism, the pylethrombosis.

The state of the liver is worthy of note. Though shrunken, particularly in left lobe, the greater part of the organ was smooth and not in the least cirrhotic. In the few instances of chronic occlusion which have been reported, the condition has been variable. In Cruveilhier's case<sup>1</sup> in which the obstruction must have lasted for years, the organ is described as smooth and healthy. In others it has been cirrhotic. Solowieff<sup>2</sup> has produced a fibroid condition of the liver by inducing occlusion of portal branches in the dog, but that this is not an invariable sequence, in man, is shown by this, as well as other cases. Nor is there any good anatomical reason why it should occur. After complete exclusion of portal blood from the organ, the lobular capillary plexus continues filled, as the venules which collect the blood from the capillaries of the hepatic artery empty directly into the portal interlobular vessels, and the blood-supply is in this way maintained. Hence the function of the gland is not materially interfered with, and bile continues to be formed from the blood furnished by the hepatic artery, which may, as in this case, undergo a compensatory enlargement. That the arterial blood can in this way act as substitute for the portal supply is well shown by such a case as the one under consideration, which forms an interesting counterpart to the one of aneurism of the hepatic artery<sup>3</sup> which apparently demonstrated that the converse is not true, but that, as Cohnheim and Litten

<sup>1</sup> *Atlas d'anat. path.* livr. xvi. Pl. 6.

<sup>2</sup> *Virchow's Archiv*, lxii.

<sup>3</sup> *Canada Med. and Surg. Journal*, 1877, Drs. Ross and Osler.

state,<sup>1</sup> the portal blood cannot replace the hepatic if the latter be completely excluded from the organ.

There is no more interesting subject of study than the way in which channels of collateral circulation are established in occlusion of large vessels. In the case of the portal vein, numerous opportunities for this purpose are afforded in cirrhosis of the liver, in which the obliteration of many interlobular branches necessitates the development of circuitous routes, by means of which the blood-current in the portal system is equalised, and the reduced carrying capacity of its vessels counter-balanced. In some instances, so adequate is this compensation that the cirrhosis may reach an extreme grade without producing symptoms. These collateral channels have been fully described by Sappey, and are chiefly :—(1) the anastomoses between the coronary veins of the stomach and the œsophageal plexus, which discharges into the lower intercostal and azygos veins, and also communicates with the diaphragmatic vessels ; (2) veins passing in the coronary and suspensory ligaments of the liver and in the adhesions which often form between the liver and diaphragm ; (3) in some cases a small vein in the round ligament dilates enormously, and affords free communication between the portal vein and the epigastric vessels. Some regard this as a redistended umbilical vein, but Sappey states that it is one of the small vessels which he describes as the *venæ portæ accessoræ*. I have recorded an instance<sup>2</sup> of advanced cirrhosis, with great narrowing of the portal branches, in which no symptoms of increased blood-pressure existed in the portal system owing to the presence of this vein, which was as large as the little finger ; (4) certain veins, forming what is known as the system of Retzius, which, originating in parts of the intestinal canal, and anastomosing with the radicles of the portal vein, discharge into the inferior cava or its branches ; (5) the communications which exist between the superior and inferior hæmorrhoidal plexuses. In this case the collateral circulation appeared to have been carried on by the first, fourth, and fifth of these channels. There were extensive communications between the gastric and œsophageal veins, and through the latter with the azygos and liver intercostals. The *vasa brevia* and others about the fundus

<sup>1</sup> Virchow's Archiv, lxvii.

<sup>2</sup> Montreal General Hospital Reports, 1880.

of the stomach were highly developed, and joined the dense network about the cardia and the diaphragm in the immediate vicinity. Many of these branches were plugged with thrombi. Doubtless a large share in the supplementary circulation was taken by the veins of the system of Retzius and the peritoneal branches emptying into the cava. The large vessel to the left of the aorta may have been a greatly distended azygos minor which Henle figures as joining with the left iliacs, but unfortunately its connection could not be made out owing to the removal of the viscera before the nature of the lesion was suspected. The blood in it probably reached the azygos, which was of large size. In the case of obliterated vena cava, recorded in this *Journal*,<sup>1</sup> I met with a similar vein. The hæmorrhoidal plexuses were not greatly distended, but the branches of the internal iliacs, particularly on the left side, were very large, and many of them contained thrombi. The epigastric veins were not dilated.

The collateral circulation must have existed for some time perhaps for years, and was fully compensatory. The somewhat sudden onset of the final symptoms may reasonably be attributed to interference with this free circulation by the thrombi in the gastric veins, and in branches of the internal iliacs.

<sup>1</sup> Vol. xiii.



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THE MUSCULAR ANATOMY OF THE KOALA<sup>1</sup> (*Phascogaleus cinereus*), WITH ADDITIONAL NOTES. By A. H. YOUNG, M.B., F.R.C.S., *late Assistant Lecturer on Anatomy, The Owens College, Manchester.*

"If," wrote Professor Owen,<sup>2</sup> referring to the Marsupialia, "the type of a natural group of animals . . . is that which manifests the greatest number of the structural modifications peculiar to the group, and the smallest number of such as are common to other natural assemblages of mammalia, then the Koala has the best claim to typical pre-eminence." So far as the soft parts are concerned, perhaps in no part of its anatomical structure does the Koala present so many "structural modifications" as are to be found in its muscular arrangements. These are, indeed, so numerous, and of such a nature, that Professor Macalister, at the close of a short paper on the "Muscular Anatomy of the Koala,"<sup>3</sup> affirms that "this animal presents, in its muscular system, a greater number of structural divergencies from the general placental type than perhaps any other Didelphian."

With the exception of the observations of Professor Macalister, I have not been able to find any detailed description of the muscular anatomy of the Koala, though some few fragmentary and scattered references do exist in the writings of other observers; and, inasmuch as the dissection of three specimens<sup>4</sup> of the species revealed certain peculiarities of muscular arrangements and relationships not hitherto recorded, I have thought that by the publication of these I might be able to supplement the observations of Professor Macalister, and at the same time complete a fuller and more detailed account of the muscular system.

Intending, some three years ago, when occupied with the

<sup>1</sup> I have to express my thanks to Professor Boyd Dawkins, to whose kindness I am indebted for the opportunity of examining the anatomy of the Koala.

<sup>2</sup> Art. "Marsupialia," *Todd's Cyclopædia*, vol. iii. p. 329.

<sup>3</sup> *Annals and Magazine of Natural History*, 1872, vol. x. p. 127.

<sup>4</sup> The number of specimens dissected affords a reasonable hope that individual peculiarities have not been regarded as definite and characteristic structures.

dissection of the Koala, to make this animal the groundwork for more extended observations into the anatomy of the marsupials, I made careful sketches, not only of the myology, but other parts of its anatomy. These sketches are still in my possession.

In the following notes the various muscles are grouped under the customary sectional headings.

Any statements as to size of muscle are made with reference to the smallest of the Koalas dissected. This measures  $20\frac{1}{2}$  inches in length.

#### MUSCLES OF THE HEAD AND NECK.

The *platysma myoides* is a prolongation forwards of the panniculus carnosus, strengthened on its deep surface by accessory fibres derived from the anterior border of the pre-sternum. The fibres form a strong muscular sheet, and become continuous anteriorly with those of the orbicular muscles of the mouth and orbit, whilst the most anterior of the fibres form a broad *attrahens aurem*. In the Wombat the *platysma* is quite distinct from the panniculus.

The facial muscles include an *orbicularis palpebrarum*, with an inferior bony attachment consisting of a rounded *inferior palpebral* muscle, and an *orbicularis oris* which is largely formed by the prolonged fibres of the *platysma*. The *levator labii superioris alæque nasi* has its usual attachments. A *levator labii superioris vel dilator naris lateralis* from the anterior part of the zygoma also passes to the upper lip and nose. There is a well-defined *depressor labii superioris*; the *depressor labii inferioris* is partially blended with the buccinator. A strong *levator menti* extends from the alveolar border of the lower jaw to the lower lip. From the nasal bones a few scattered fibres converge towards the middle line of the nose and form a rudimentary *elevator naris*. *Buccinator* is as usual. So also is the *occipito-frontalis*.

The auricle is furnished with a series of irregularly disposed muscular fibres, which, however, evidence a tendency to the formation of antero-posterior rows; these, along with a strong bundle from the tragus to the concha, constitute the intrinsic muscles. The extrinsic muscles comprise three *attrahentes*—

superficial, middle, and deep—an *attollens*, and a bilaminar *retrahens*.

Of the muscles of mastication, Macalister<sup>1</sup> found the *external pterygoid* exceedingly small, and inseparable from the *internal pterygoid*. In my specimens it is distinct and well-developed. The *temporal* and *masseter* muscles have their usual attachments; in only one instance did I find them at all united.

The *sterno-mastoid* is quite separate from the cleido-mastoid, and considerably exceeds it in size. Its insertion is solely into the outer half of the occipital ridge, and its origin from the manubrium sterni-posterior to the clavicular articulation.

The *cleido-mastoid* is smaller than the preceding; it arises from the sternal end of the clavicle, and is inserted into the occipital ridge. A small slip of fibres, detached from the edge of the trapezius, runs down to the clavicle, or, passing over this bone, to the pectoral fascia; this corresponds apparently to the accessory slip found by Meckel<sup>2</sup> in the Opossum. In this latter animal I found the cleidi mastoid partially united to the trapezius.

A thick *levator scapulae* springs from the front of the lateral mass of the atlas, in common with the first slip of the serratus magnus, and passes to the lower half of the scapular spine, extending but slightly on to the acromion.

The *sterno-hyoid*, *omo-hyoid*, and *digastric* muscles have a common insertion into the mandible; the two former have no attachment whatever to the hyoid bone, and consequently have no influence on its movements. The posterior belly of the *digastric* arises from the paroccipital process and ends at a tendinous inscription, which separates it from the anterior belly, where it is joined by the sterno-hyoid. In the angle between these two muscles the omo-hyoid is attached. A Y-shaped tendinous intersection marks the junction of the three muscular elements. From this point a strong muscular bundle, which corresponds in position and general appearance to the anterior belly of the digastric, though it greatly exceeds the posterior in size, passes to an insertion into the inferior border of the lower jaw, which extends from one inch behind the symphysis as far as the angle. The *sterno-hyoid* arises from the

<sup>1</sup> *Loc. cit.* p. 128.

<sup>2</sup> *Anat. Comp.* vol. vi. pp. 166, 167.

upper (dorsad) surface of the sternum, at the junction of the second and third segments, and receives also accessory fibres from the second and third costal cartilages. It has no clavicular attachment. The *omo-hyoid* arises from the anterior margin of the scapula near its superior angle. It presents no trace of a tendinous intersection before it reaches the digastric. Whilst the greater part of the fibres of this muscle terminate as just noted, some few pass beneath the digastric, and, joined by others from the tendinous intersection of the latter, run along the inferior surface of the tongue as the *lingualis*. This remarkable arrangement of these muscles was constant in the different specimens examined. So far as I am aware it has not been met with in other marsupials.

The *sterno-thyroid*, from sternum to thyroid cartilage as usual, is with difficulty separable from its fellow of the opposite side; a separate *thyro-hyoid* is prolonged to the great cornua of the hyoid bone.

The *mylo-hyoid* is arranged in the usual manner, but does not reach back to the hyoid bone.

*Genio-hyoid* is long and riband-like. Its fibres extend from the symphysis of the lower jaw to the great cornua of the hyoid bone.

The *genio-hyo-glossus* is of considerable interest; extending from the genial tubercles to the hyoid bone and thyro-hyoid membrane, the anterior half of the fibres pass to the tongue in the ordinary manner, whilst the posterior half, inseparable from those of the opposite muscle, form a muscular sheet; this constitutes the greater portion of the walls of a tubular cavity, about three inches in length, which extends from the back of the tongue to the larynx. The wall of this cavity is completed by the enormously elongated soft palate.<sup>1</sup>

*Hyo-glossus*.—Beneath the *genio-hyoidei*, and bridging over the *genio-hyo-glossi*, is a transverse muscular band which is attached laterally to the substance of the tongue. Macalister<sup>2</sup> described this band as an aberrant slip of the *mylo-hyoid*. It appears to represent rather the *hyo-glossi*, which, instead of being attached to the hyoid bone, do not descend so far, but

<sup>1</sup> "Anatomy of the Koala," *Journ. of Anat. and Phys.* vol. xv. p. 466.

<sup>2</sup> *Loc. cit.* p. 129.

join together and form the bridge-like muscle just referred to. As described by Macalister, the *styloid* group of muscles consist "of a single sheet, the hinder fibres of which pass to the pharynx, the middle to the cerato hyal, under the stylo-hyoid ligament, and the anterior to the side of the tongue."

In one specimen only, I found what appeared to be an aberrant slip of the stylo-glossus reaching from the fascia covering the internal pterygoid to the side of the tongue.

*Scaleni*.—These muscles are all situated behind the brachial plexus. The *scalenus anticus* must therefore be regarded as wanting. *Scalenus medius* is from the transverse processes of the posterior three cervical vertebræ to the first rib. *Scalenus posticus*, which is easily divisible into two, is from the transverse processes of the cervical vertebræ—except the first and last—to the first three ribs.

The muscles of the cervical subvertebral region are more or less inseparable from each other. So far as they can be distinguished, the *rectus capitis anticus major* is by far the largest of the group; it reaches from the bodies of the anterior three dorsal vertebræ and transverse processes of all the cervical to the basi-occiput. The *rectus capitis anticus minor* cannot be separated from the major.

*Longus colli* is comparatively small unless some of the fibres considered as part of the *rectus capitis anticus major* more rightly belong here. It consists of fibres extending between bodies and transverse processes of the anterior three dorsal and cervical vertebræ.

*Rectus capitis lateralis* stretches from the transverse process of the atlas to the paramastoid process.

*Splenius* arises from the spines of the posterior five cervical and anterior five dorsal vertebræ. Its insertion is partly by three fleshy slips into the anterior three cervical transverse processes, forming a *splenius colli*, and partly into the occipital crest, thus constituting a *splenius capitis*. There is no cervical insertion in the Opossum or in the Kangaroo.

*Complexus*.—This muscle arises from the anterior six dorsal transverse processes and from the articular processes of the last five cervical vertebræ. Its insertion is into the occiput. There is a strong bilaminar *rectus capitis posticus major*, and a distinct

*rectus capitis posticus minor*. The *superior oblique* is a double muscle; the *inferior oblique* is large, and has an origin from the second, third, and fourth cervical vertebræ.

*Spinalis colli* is well marked.

#### MUSCLES OF THE BACK, THORAX, AND ABDOMEN.

*Panniculus carnosus*.—This muscle is of considerable extent; weak and thin posteriorly, it becomes stronger as it passes downwards and forwards on the sides and ventral aspect of the trunk; part of the fibres are prolonged, as the platysma, whilst the rest converge towards the axilla, and are inserted into the humerus.

In the region of the abdomen of the female Koala the muscular fibres almost surround, sphincter-like, the mouth of the marsupial pouch. This is even better marked in the Wombat.

Martin<sup>1</sup> describes also a set of transverse fibres which pass over the anterior extremities of the marsupial bones. These are much more strongly developed in the female than in the male.

The *trapezius* is single and undivided. Clavicular attachment is either very limited or entirely wanting. The origin of the muscle extends from the occiput as far back as the eighth dorsal spine; its insertion is into the spine and acromion process of the scapula, whilst the anterior fibres are continued onwards, to end in the deep fascia covering the deltoid. A muscular slip, like a detached bundle of fibres from the anterior margin of the trapezius, is either inserted into the clavicle, or, passing over it, ends in the pectoral fascia. (See *Cleido-mastoid*.)

The *latissimus dorsi*, from the lumbar aponeurosis and spines of six dorsal vertebræ to the ordinary humeral insertion, gives off a well-marked *dorso-epitrochlearis* to the olecranon. As in the Opossum, the *latissimus dorsi* in Koala has no costal attachment.

The *rhomboides* consists of an undivided single muscle, which arises from the inner half of the occipital ridge, and reaches as

<sup>1</sup> "Notes on the Anatomy of the Koala," *Proc. Zool. Soc.* part iv. 1836, p. 109.

far back as the third dorsal spine. Its insertion is into the vertebral border of the scapula.

Superior and inferior *serrati postici* are only separable by the different directions of their fibres; the superior is by far the most extensive.

*Erector spinæ*.—The common extensor of the back is subdivided in the usual manner.

A small *sacro-lumbalis*, attached to the posterior three ribs, is prolonged to the second rib by a well-defined *musculus accessorius*. As in most marsupials, there is no *cervicalis ascendens*.

The *longissimus dorsi* is the largest of the subdivisions of the erector spinæ. It has the ordinary attachments, and, as is usually the case where no *cervicalis ascendens* is present,<sup>1</sup> is prolonged into the neck, where it is attached to the transverse processes of the posterior three vertebræ.

The *transversalis cervicis* and *trachelo-mastoid* have a common origin from the four or five anterior dorsal transverse processes, and from the five posterior articular processes of the cervical region. The insertion is into the transverse processes of all the cervical vertebræ, whilst a riband-like slip to the occiput constitutes the *trachelo-mastoid*.

*Spinalis dorsi* passes to the spines of the anterior dorsal vertebræ.

*Levatores costarum* are strongly marked; they are continuous with the external intercostals.

The *multifidus spinæ*, *interspinales*, and *intertransversales* are as usual.

*Serratus magnus* arises from the transverse processes of the posterior six cervical vertebræ and from the anterior nine ribs. It is inserted into the superior costa of the scapula.

The *intercostal* series of muscles have their ordinary attachments. Well-defined *subcostales* are present.

The *triangularis sterni* extends from the third, fourth, and fifth sternal segments to the cartilages of the third, fourth, fifth, and sixth ribs.

The *diaphragm* has the ordinary attachments. The orifice of the vena cava exists as usual in the cordiform tendon; the aorta passes between the tendinous crura, whilst the œsophagus

<sup>1</sup> Meckel, *Anat. Comp.* tom. vi. p. 149.

is situated between the muscular fibres derived from the crura, but behind their junction, so that, like the aorta, it is practically behind the diaphragm.

Of the abdominal muscles the *external oblique* is very strong. The *internal oblique* is easily separable from the *transversalis*.

The arrangement and attachments of these muscles correspond very closely with what is found in most marsupials. Professor Humphry<sup>1</sup> notes, however, that "in the Wombat a few of the hindmost fibres of the internal oblique are connected with the marsupial bone. This is not the case in the Koala or in the Opossum."

In the Koala, however, the internal oblique is noteworthy by reason of the presence of four or five well-marked tendinous intersections which exist in its muscular portion. These are in serial continuity with the ribs. Thus another example of the persistence of intermuscular septa in one of the higher vertebrates is added to those more commonly met with.

The *rectus abdominis* reaches as far forwards as the first rib. Tendinous intersections are eight in number; they are well marked, and distributed along the whole extent of the muscle. The first tendinous intersection is large, and in consequence the anterior part of the muscle is almost differentiated as a separate *rectus thoracis*.<sup>2</sup>

*Pyramidalis*, as usual very voluminous, almost conceales the abdominal rectus. It arises from the inner edge of the marsupial bone, and as in the Wombat, extends forwards and inwards towards the middle line, becoming more or less blended with the rectus. Some few fibres are also attached to the posterior segment of the meso-sternum and to the cartilage of the last true rib.

The *quadratus lumborum* can hardly be said to exist as a separate muscle. So Meckel<sup>3</sup> found also in the Kangaroo. A series of intertransversales muscles are present in connection with the lumbar transverse processes, and the most posterior of these receives a few fibres from the iliac crest; these may perhaps be regarded as a rudimentary ilio-lumbar element of a

<sup>1</sup> *Observations on Myology*, p. 16.

<sup>2</sup> "On the Musculus Sternalis," *Journ. of Anat. and Phys.* vol. i.

<sup>3</sup> *Loc. cit.* p. 198.



*quadratus lumborum*. Above (dorsad) these are the deep fibres of the *erector spinæ*, which pass to the last rib.

Macalister<sup>1</sup> describes an ordinary *quadratus* with its three usual component parts.

The *psosas parvus* is large and strong, it considerably exceeds in size the *psosas magnus*; the latter is joined by a bi-partite *iliacus* and the two muscles take their usual insertion.

The *levator ani* is arranged as usual.

*Coccygeus*, inserted into sacrum and anterior caudal vertebræ, is continuous posteriorly with the depressor caudæ.

*Sphincter cloacæ* has been previously described.<sup>2</sup>

*Muscles of the Tail*.—Though the tail of the Koala is so rudimentary, the muscular elements are easily distinguished. There is a strong *depressor caudæ*, which arises from the front of the sacrum and, receiving fibres from the *coccygeus*, terminates at the last coccygeal vertebra; the *elevator caudæ* is represented by the posterior fibres of the deep muscles of the back.

*Lateralis caudæ*, very strong, arises from the ischial tuberosity, and passes to the transverse processes of the anterior three caudal vertebræ.

#### MUSCLES OF THE FORE-LIMBS.

The *pectoralis major* is bilaminar. It springs from the whole length of the sternum and from the abdominal aponeurosis, and is inserted into the delto-pectoral ridge. In one Koala I found in addition a small clavicular slip.

*Pectoralis minor* is from the sternal ends of the third, fourth, and fifth costal cartilages to the great tuberosity of the humerus and capsule of the shoulder-joint.

A *pectoralis minimus* extends from the first and second costal cartilages to the great tuberosity of the humerus.

*Pectoralis quartus*.—Macalister<sup>3</sup> describes a muscle under this name as springing from the costal cartilages. Owen<sup>4</sup> also refers to the muscle in the Bandicoot, and regards it as a dismemberment of the *pectoralis major*. I found it arising from the abdominal aponeurosis and inserted along with the panniculus, of which it appeared a part.

<sup>1</sup> *Loc. cit.* p. 131.

<sup>2</sup> *Journ. of Anat. and Phys.* vol. xiii. p. 312.

<sup>3</sup> *Loc. cit.* p. 130.

<sup>4</sup> *Anat. of Vert.* vol. iii. p. 12.

Though in most marsupials separate pectoral muscles are easily distinguishable, there appears to be considerable differences in the light in which these are regarded, *e.g.*, Meckel<sup>1</sup> groups them together under the name of the pectoralis major, which he describes as consisting of three parts—one superficial, from sternum or from clavicle and sternum; the others deep, arising separately from the side of the sternum, as in the Opossum, or from the costal cartilages, as in the Kangaroo; the pectoralis minor is therefore regarded as absent. Vrolik, however, speaks of the humeral attachment of the pectoralis minor in the Kangaroo, whilst in the Bandicoot a coracoid attachment of the pectoralis major is regarded as representing it.

*Subclavius* is strong; it possesses the ordinary attachments, and is covered by a well-marked costo-coracoid membrane. I found in one of my specimens a small slip, apparently a dismemberment of the subclavius, extending from the first costal cartilage to the coracoid process of the scapula.

The *deltoid* is comparatively small. It consists of two distinct portions—clavicular and scapular—which are inserted together into the lower part of the delto-pectoral ridge. The origin of the clavicular portion corresponds to the outer half of the clavicle. The scapular moiety arises from the acromian process, from the posterior border of the scapular spine, and also from the investing fascia of the *infraspinatus* muscle, but no division is discernible between the acromial and scapular fibres.

The tri-partite character of the deltoid, so common in clavicate marsupials,<sup>2</sup> does not exist in Koala.

The scapular muscles are well developed, fleshy masses. Those occupying the dorsal and ventral aspects of the bone project beyond its margins and take additional origins from inter-muscular septa, separating them from each other. In other respects there is nothing unusual in the attachments of these muscles. The *supraspinatus* is somewhat smaller than the *infraspinatus*. The *subscapularis* is large and thick. It extends from the venter scapulæ to a broad and strong insertion into the small tuberosity of the humerus, some few fibres taking an attachment to the capsule of the shoulder-joint.

The *teres major* arises from the upper half of the axillary

<sup>1</sup> *Loc. cit.* p. 273.

<sup>2</sup> Owen, *Anat. of Vert.* vol. iii. p. 12.

border of the scapula; its insertion is behind, and closely connected with, that of the latissimus dorsi.

Macalister<sup>1</sup> found the *teres minor* small but distinct, and with an unusually wide fleshy insertion. In only one of my specimens was it distinguishable as a separate muscle from the *infraspinatus*. In this it was very small, but had the usual attachments. The absence of the *teres minor* as a distinct muscle appears to be general amongst marsupials.<sup>2</sup>

The *coraco-brachialis* consists of two distinct parts. Of these, one arises by a long tendinous origin from the tip of the coracoid process, becomes muscular, passes over the insertions of the *teres major* and *latissimus dorsi*, and terminates by an extensive insertion into the internal border of the humerus; the insertion extends from that of the great dorsal muscle above as far down as the internal condyle. This portion of the muscle corresponds to what obtains in the monotremes,<sup>3</sup> and represents the long and middle varieties of the muscle as described by Professor Wood.

The second portion of the muscle is a well-marked *coraco-brachialis brevis vel superioris* (Wood). This springs fleshy from the coracoid process, winds round the lower border of the subscapularis, and is inserted into the shaft of the humerus above the *teres major*.

The *biceps*, as usual in marsupials, consists of gleno-ulnar and coraco-radial portions. Their origin, which is conjoined, is broad and tendinous; this soon gives place to two separate muscular bellies, a superficial, which is inserted by a flattened tendon into the radial tuberosity, and from which is given off the bicipital fascia, and a deep, which has a fleshy insertion into the coronoid process of the ulna.

The *brachialis anticus* springs from almost the whole of the outer surface of the shaft of the humerus, winds round to the front of the elbow-joint, and is inserted in common with the gleno-ulnar moiety of the *biceps*. In one Koala the muscle was divided at its insertion, a few of the fibres passing to the radius with the coraco-radial part of the *biceps*.

The *triceps* has the usual origins and insertion. Its scapular

<sup>1</sup> *Loc. cit.* p. 180.

<sup>2</sup> Meckel, *Anat. Comp.* vol. vi. p. 278.

<sup>3</sup> Wood, "Muscular Variations," *Journ. Anat. and Phys.* vol. i. p. 44.

head arises from the lower half of the axillary border of the scapula; the external and internal heads from the posterior surface of the humerus; the external having by far the most extensive attachment.

*Anconeus*.—Two in number. The *external* from the outer condyle of the humerus and shaft of bone immediately above to the outer side of the olecranon and upper third of the shaft of the ulna; and the *internal* (*anconeus epitrochlearis*) a small thin muscle, the fibres of which extend from the inner condyle of the humerus almost directly backwards to the internal side of the olecranon.

Of the muscles of the forearm, the *supinator longus* is remarkable for its size. It arises by an aponeurotic origin from the external border of the humerus, reaching from the neck of that bone down to the supinator ridge, from which latter fleshy fibres arise. A large portion of the muscle is consequently situated in the upper arm; inferiorly the muscular fibres end on a strong tendon which winds round the outer surface of the lower end of the radius, as over a pulley, and passes forwards to be inserted into the scapho-lunar bone, as in the Opossum. By reason of its carpal insertion the supinator longus acts as a radial flexor of the wrist, but it is none the less a powerful supinator of the forearm, for in the Koala, it is to be noted, the movements of pronation and supination are not simply movements limited to the superior and inferior radio-ulnar articulations, but also, and to an important extent, occurring at an enarthrodial joint formed by the rounded head of the ulna, which is received into a concavity formed by the cuneiform and pisiform bones.

The *supinator brevis* has no ulnar attachment; the posterior interosseous nerve passes entirely beneath the muscle.

The *pronator radii teres*, from the internal condyle of the humerus solely, passes to the middle third of the radius. The *pronator quadratus* is limited to the lower fifth of the forearm; it is, therefore, comparatively small, in this respect agreeing closely with the Kangaroo,<sup>1</sup> but differing widely from many marsupials, *e.g.*, Opossums, *Perameles*,<sup>2</sup> &c., in which it occupies the whole length of the interosseous space.

<sup>1</sup> Meckel, *Anat. Comp.* vol. vi. p. 302.

<sup>2</sup> Owen, *Anat. of Vert.* vol. iii. p. 13.

*Extensores carpi radialis*.—The arrangement of the radial extensors of the carpus appears to vary. Macalister<sup>1</sup> found two muscles quite separate and distinct, and such was the condition in one of my specimens. In the two other Koalas, however, I found the two muscles fused into a single mass. Meckel<sup>2</sup> describes two separate extensors in the Opossum, whilst, according to the same authority, the Kangaroo presents but one.

The *extensor carpi radialis longior* is very small. It extends from the supinator ridge to near the middle of the radial border of the second metacarpal bone.

The *extensor carpi radialis brevior* is from the external condyle to the third metacarpal bone—low down on its radial border.

There is an *extensor carpi ulnaris* from the external condyle and upper part of the posterior border of the ulna to the base of the fifth metacarpal.

The *extensor communis digitorum* is distributed to all the digits. It arises from the outer condyle and from the inter-muscular and investing fasciæ, and terminates by five slender tendons. Of these the two radially situated pass together beneath the posterior annular ligament to be distributed to the pollex and index digits respectively. The remaining tendons pass in company through a separate compartment of the annular ligament, and reaching the back of the hand form a tendinous expansion from which slips are given to each of the three ulnar digits, where they terminate in the usual manner.

The three ulnar digits receive also the tendons of a deeper muscular mass, which may be regarded as an *extensor secundus digitorum*; this muscle has an extensive fleshy origin from the shaft of the ulna; the muscular mass divides so high up as to form almost two distinct muscles, of which one terminates in a single tendon which passes to the third digit, and joins the expansion of the common extensor tendon opposite the metacarpo-phalangeal articulation; this constitutes an *extensor medii digiti*, whilst the other is distributed to the fourth and fifth digits, and represents the *extensor minimi digiti*.

A strong *extensor ossis metacarpi pollicis* rises from the orbi-

<sup>1</sup> *Loc. cit.* p. 130.

<sup>2</sup> *Anat. Comp.* vol. vi. p. 309.

cular ligament, from the greater part of the posterior surface of the shaft of the radius, and from the radial half of the interosseous membrane; it is inserted into the trapezium and adjoining part of the metacarpal of pollex.

Professor Macalister<sup>1</sup> describes an *extensor secundi internodii* arranged as usual. In none of my specimens does this exist. It appears to be wanting also in the majority of marsupials.<sup>2</sup> The thumb has no special extensor of the phalanges beyond the slip derived from the common extensor.

There is no special *extensor indicis*.

The *flexor carpi radialis* is small; its origin is from the internal condyle, and its insertion is into the proximal extremity of the third metacarpal.

The *flexor carpi ulnaris* is more strongly developed; its condyloid and olecranoid heads soon unite; the muscle ends by a rounded tendon, which passes to the pisiform bone; prolongations are given off to the unciform and fifth metacarpal bones.

There is a well defined, though slender, *palmaris longus*.

*Flexores digitorum*.—The superficial and the deep form one large irregular muscular mass. This arises by distinct heads from the bones of the arm. From the humerus one head springs from the anterior surface of the lower extremity immediately below the epicondyloid foramen, and a second from the internal condyle beneath the other flexors. The ulnar origin is from the inner surface of the olecranon and upper two-thirds of the shaft of the bone, the radial is from the greater part of the shaft and adjoining interosseous membrane. The fibres so derived are soon distinguishable as forming two strata, a superficial and a deep.

The superficial stratum terminates in a single tendon which passes to the terminal phalanx of the fifth digit.

The deeper stratum terminates on a strong flat single tendon which divides opposite the wrist-joint into four; these are distributed to the first four digits, including the pollex, and are inserted into the distal phalanges. A fascial slip connects the tendons of the fourth and fifth digits together.

The tendons and muscular structures just described represent

<sup>1</sup> *Loc. cit.* p. 131.

<sup>2</sup> Meckel, *Anat. Comp.* vol. vi. p. 329.

the *flexor profundus digitorum* and *flexor longus pollicis* conjoined.

The *flexor perforatus* consists of a feebly-marked muscle, which is derived from the adjacent surfaces of the muscular strata of the general flexor mass, and ends by four extremely delicate tendons, which, after being perforated by the corresponding deep tendons, are attached to the second phalanges of the four ulnar digits.

This muscle, whilst it may correspond to a *flexor sublimis*, appears to be more closely allied to the *flexor brevis manus*, first described by Mivart and Murie<sup>1</sup> in *Hyrax capensis*.

Macalister<sup>2</sup> found the *flexor sublimis* springing from the front of the general flexor mass in his Koala. Such appears to be the condition in the Opossum,<sup>3</sup> and also in *Perameles lagotis*.<sup>4</sup> In the latter animal, the superficial tendons come off low down from the common tendon, and therefore have no muscular bellies.

In the Kangaroo<sup>5</sup> the superficial tendons appear to be entirely absent.

*Lumbricales*, two in number, arise from the opposite sides of the deep flexor tendon of the middle digit just at its origin, and are distributed in the usual manner to the third and fourth digits.

The intrinsic muscles of the hand of the Koala have already been referred to in a previous communication to this *Journal*<sup>6</sup> Whilst no general description of the muscles was given, it was pointed out that in their disposition they agreed closely with the arrangement met with in many other marsupials, and regarded by Dr D. J. Cunningham<sup>7</sup> as the "typical arrangement" in the mammalia generally.

Before referring more particularly to the muscles in the hand of the Koala, I must again point out the peculiarity of the disposition of the digits in the fore-limb of this animal. They form two groups, of which one, including the thumb and index finger, is opposable to the other, this latter comprising the three remaining digits.

<sup>1</sup> *Proc. Zool. Soc., Lond.* 1865, p. 341.

<sup>2</sup> *Loc. cit.* p. 130.

<sup>3</sup> Meckel, *Anat. Comp.* vol. vi. p. 336.

<sup>4</sup> Owen, *Anat. of Vert.* vol. iii. p. 13.

<sup>5</sup> Meckel, *loc. cit.* p. 336.

<sup>6</sup> "The Intrinsic Muscles of the Marsupial Hand," vol. xiv. p. 158.

<sup>7</sup> *Journ. of Anat. and Phys.* vol. xii. p. 434; see also vol. xiii. p. 1.

The index digit, in point of fact, constitutes a second thumb; and, in conformity with its unusual freedom of action, its special muscles are well developed. Moreover, as the result of the arrangement of the digits into two groups, the middle line of the hand no longer passes through the *third* digit, but along the *fourth*. The muscles of the hand are correspondingly modified, and hence, so far as regards their insertions, the arrangement is very different to what is ordinarily found. The abductors and adductors act as usual from or to a middle line, but this, in the case of the Koala, passes along the fourth digit.

The intrinsic muscles consist of—

1. A *plantar* layer of *adductores*.
2. An *intermediate* layer of *flexores breves*.
3. A *dorsal* layer of *abductores*.

*Plantar Layer*.—This consists of three adductor muscles, which are distributed to the first, second, and fifth digits respectively. It includes, therefore, (1) an *adductor pollicis*, (2) an *adductor indicis*, and (3) an *adductor minimi digiti*. These muscles evidence the usual marsupial tendency for a common central origin, and all arise from a tendinous raphæ, situated in the palm, which, however, does not correspond to the true mesial line, but runs along the palmar aspect of the third metacarpal bone. The adductor pollicis and the adductor minimi digiti are almost of equal size. They arise from the upper half of the tendinous raphe, and from the adjoining ligamentous structures, and are inserted into the proximal phalanges of their respective digits in the usual manner. A delicate muscular slip, derived from the lowest fibres of the adductor minimi digiti, passes to the ulnar side of the fourth digit.

The adductor indicis arises from the lower half of the tendinous raphe, immediately below the adductor pollicis, and passes to the ulnar side of the first phalanx of the index finger, where it joins the expansion of the extensor tendon.

*Intermediate Layer*.—The flexores breves, except in the case of the fifth digit, consist of a series of paired muscular bundles, on the palmar aspects of each metacarpal bone. They are quite distinct from the dorsal and plantar layers.

The *flexor brevis pollicis* is very small. Its muscular bellies have a common origin from the annular ligament and from the



trapezium. The outer moiety is inserted along with the abductor pollicis, the inner with that of the adductor.

The *flexor brevis indicis* is large and strong. Its outer portion is flat and triangular, and is concealed in great part by the adductor pollicis, which it almost equals in size, and resembles closely in shape. By its base it springs from the deeper aspect of the tendinous raphæ of the palm, and from the carpal extremity of the second metacarpal bone; by its apex it is inserted on the radial side of the proximal phalanx of the index finger. The ulnar portion of the flexor brevis indicis arises from the carpal end of the third metacarpal, and joins the insertion of the adductor indicis.

*Flexor brevis minimi digiti* is only represented by a slender ulnar portion, which takes origin from the anterior annular ligament, and from the unciform bone, and is inserted with the abductor minimi digiti. The rest of the flexor group consists of strong muscular bundles—two to each of the third and fourth fingers—which arise from the proximal extremities of the corresponding metacarpal bones, and are inserted into the proximal phalanges of the respective digits, being connected also with the expansions of the extensor tendons. Small sesamoid bones exist in connection with all the flexors.

*Dorsal Layer.*—This group includes the *abductor pollicis*, the *abductor indicis*, the *abductor minimi digiti*, and the *dorsal interossei*.

The abductors of the thumb and little finger have their usual attachments and relative positions.

The abductor indicis corresponds to the first dorsal interosseous. It is a single-headed muscle, very similar to that figured in the Wallaby,<sup>1</sup> though relatively stronger.

Of the remaining dorsal interossei, the third and fourth are single-headed muscular bundles, which act as abductors of the fourth finger, arising from its metacarpal, and being inserted into its proximal phalanx. The second dorsal interosseous is somewhat more complex. It arises by distinct and separate origins from the posterior carpo-metacarpal ligaments, from the upper portion of the second metacarpal bone, and from the radial border of the shaft of the third metacarpal. The fibres

<sup>1</sup> *Journ. of Anat. and Phys.* vol. xiv. Plate VII., figs. 5 and 6.

end inferiorly on a slender arched tendon, the extremities of which are attached to the adjacent sides of the proximal ends of the first phalanges of the second and third digits.

An *opponens pollicis* does not exist. There is a distinct and separate *opponens minimi digiti*.

### MUSCLES OF THE HIND-LIMBS.

The *gluteus maximus* has no iliac attachment. Strong and coarse, the muscular bundles take their origin from the sacral and coccygeal vertebræ, and have an extensive insertion into the shaft of the femur posteriorly. In marsupials generally the *gluteus maximus* is of relatively larger size, and usually possesses an iliac origin.

The *gluteus medius* considerably exceeds in size the muscle last described. It arises from the crest and gluteal surface of the iliac bone, and through the aponeurosis covering the erector spinæ from the sacral spines; the insertion is into the great trochanter. In all my specimens the *gluteus medius* was more or less bilaminar, whilst in the case of one the lamination was complete, and two separate muscles existed.

*Gluteus minimus* is single and undivided. In the Kangaroo Meckel<sup>1</sup> describes it as separable into two parts.

A *tensor vaginæ femoris* is not distinguishable.

*Pyriiformis*.—This muscle is of large size. Its attachments are as usual. The sciatic nerve leaves the pelvis above the muscle.

The *gemelli*, arranged as usual, are united beneath the extra-pelvic portion of the obturator internus. They join the tendon of the latter, and are inserted with it into the trochanteric fossa immediately above the insertion of the obturator externus. According to Meckel<sup>2</sup> the *gemelli* are entirely wanting in the Kangaroo. They exist in the Opossum much as in Koala.

The *obturator externus* is strong and fleshy; it possesses the customary attachments.

The *quadratus femoris* has no ischial origin. It arises solely from the great sciatic ligament and the transverse process of the first coccygeal vertebræ, and is inserted into the posterior border of the great trochanter.

<sup>1</sup> *Loc. cit.* p. 360.

<sup>2</sup> *Loc. cit.* p. 364.

The hamstring muscles, three in number, are quite separate and distinct, thus differing from the Kangaroo. They all arise by single heads from the tuber ischii, much as in man.

The *biceps* has no femoral origin and is a single muscle, the ischio-fibular element of most mammals. From the origin the muscular bundles diverge, forming a flattened triangular sheet, and terminate by an insertion into the deep aponeurosis of the leg on its fibular side, the insertion extending for a distance of  $2\frac{1}{2}$  inches, but not reaching the sheath of the tendo-achillis, as is the case in *Perameles lagotis*. The character and extent of the insertion of this muscle must be associated with the semi-flexed attitude of the limb.

The *semi-tendinosus* arises in common with the biceps, and is inserted into the internal surface of the shaft of the tibia immediately behind the gracilis.

The *semi-membranosus* has a broad flattened insertion into the internal tuberosity of the tibia, and also into the quadriceps tendon and deep fascia of the leg.

There is no *bicipiti accessorius*, such as exists in the Opossum (*Didelphis virginiana*).

*Sartorius*, with its usual origin, expands inferiorly, and has a broad insertion into the tendon of the quadriceps, and into the deep fascia of the leg. The extensor action of this muscle has been pointed out in many marsupials,<sup>1</sup> e.g., Kangaroo, Bandicoot, and one of the Dasyures (*D. macrurus*); it exists also in the Opossum.

The *gracilis* has a broad origin from the arch and symphysis of the pubes. In one specimen some few fibres were attached to the marsupial bone, thus resembling the same muscle in *Ornithorhyncus*.<sup>2</sup> Insertion is into the internal surface of the shaft of the tibia.

*Pectineus*.—Single and rounded at its origin, which is almost entirely from the external inferior angle of the marsupial bone, this muscle soon bifurcates and has a double insertion into the shaft of the femur, the lowest of which is prolonged down to the middle of the bone. The muscle is similarly disposed in the Opossum.

<sup>1</sup> Art. "Marsupiala," *Todd's Cyclop.* vol. iii. p. 290.

<sup>2</sup> Meckel, *loc. cit.* p. 396.

Triceps adductor femoris, regarded as a single muscular mass, more or less divisible into different strata, arises from the innominate bone solely, and is inserted along almost the whole length of the posterior surface of the shaft of the femur.

Martin,<sup>1</sup> Owen,<sup>2</sup> and Macalister<sup>3</sup> all affirm an attachment to the external angle of the marsupial bone of the triceps adductor femoris. In all my specimens the external angle of the marsupial bone was occupied by the pectineus, whilst the origin of the gracilis intervened between the adductor mass and the marsupial bone.

The quadriceps extensor includes the usual components. *Rectus femoris* has the common double origin; the *vastus externus* constitutes by far the greater part of the "trifemoro rotuleus;" the *vastus internus* and *crureus* are inseparable. They all terminate in a short broad tendon, which is inserted directly into the superior extremity of the tibia. There is no patellar ossification.

*Tibialis anticus* is inserted solely into the ento-cuneiform bones. It has an extensive origin from the tibia and interosseous membrane.

The *extensor longus digitorum*, as in most marsupials, possesses no femoral attachment. It arises from the external tibial tuberosity, the head of the fibula, and also from the intermuscular fascia. The tendons of insertion are distributed to the four outer toes, where they terminate in the usual manner. The muscular mass is easily separable into two parts, and in this respect Koala appears to differ from such marsupials as have been examined. In the *Ornithorhynchus* the muscle is composed of two parts.

There is a separate *extensor hallucis* from the fibula to the great toe.

*Peronei*.—Four separate peroneal muscles are distinguishable, the origin of all being fibular. A *peroneus longus* runs to the metatarsal of the great toe; a *peroneus brevis* is inserted into the fifth metatarsal; a *peroneus quarti metatarsi* (Macalister) is distributed to the fourth toe, joining the extensor expansion;

<sup>1</sup> *Proc. Zool. Soc.* part iv., 1836, p. 110.

<sup>2</sup> *Todd's Cyclop.* vol. iii. p. 288; and *Anat. of Vert.* vol. iii. p. 9.

<sup>3</sup> *Loc. cit.* p. 132.

and a *peroneus quinti metatarsi* (Macalister) is similarly distributed to the fifth toe. In the Opossum these muscles are similarly arranged, but there is also a femoral origin; whilst the *peroneus quarti metatarsi* is distributed to the second and third toes, as well as the fourth, and forms an *extensor secundi digitorum*. According to Meckel<sup>1</sup> there are but two peroneal muscles in the Kangaroo.

*Extensor brevis digitorum* is feeble and ill-developed. Two slender tendons only originate from it. These are distributed to the second and third toes.

*Gastrocnemius*, well developed, arises by two heads, in the external of which a fabella exists. Its insertion below is into the os calcis.

The *soleus* is not present as a separate muscle. Macalister states that it "is inseparable from the external head of the *gastrocnemius*;" the muscle in question appears rather to be represented by certain superficial fibres of the common flexors of the toes, which are prolonged downwards as the *flexor perforatus*. The position of the *plantaris* muscle bears out this view. *Plantaris* arises in common with the external head of the *gastrocnemius*, and terminates usually in the plantar fascia. In one specimen, however, I found it joining the *tendo-achillis*.

The *tibialis posticus* is double (so in the Beaver<sup>2</sup>), both parts have a fibular origin—one is inserted into the scaphoid, the other into the entocuneiform and the sesamoid at the base of hallux. In *Phalangista vulpina* the muscle is single, but terminates in two tendons; in *Didelphis virginiana* it is single throughout.

The *flexor longus digitorum* and the *flexor longus hallucis* are inseparable. The conjoined muscle arises from the upper three-fourths of the fibula, and terminates at the ankle in a stout flat tendon, which expanding somewhat in the sole gives off five tendons, one to each toe. Some superficial fibres of the muscular mass end differently. They pass down and form two small fleshy bellies, from each of which two slender tendons proceed. The four tendons so derived pass to the four outer toes, and are perforated by the underlying deep tendons; so the *flexor perforatus* is constituted.

*Lumbricales* are four in number.

<sup>1</sup> *Loc cit.* p. 412.

<sup>2</sup> Meckel, *loc. cit.* p. 424.

*Popliteus* or *pronator tibiæ* occupies the whole length of the interosseous space, passing between the bones of the leg, and having no femoral attachment. It has already been described in a former communication.<sup>1</sup>

There is no *flexor accessorius* in the foot; but a muscular bundle, which extends from the os calcis to a fibro-cartilaginous backward prolongation of the plantar fascia is regarded by Macalister as similar to it in nature. The intrinsic muscles of the foot consist of (1) a *plantar layer*, which includes an *adductor hallucis*, an *adductor minimi digiti*, and an *adductor* of the syndactyle second and third toes; (2) an *intermediate layer*, comprising a series of muscular bellies, two to each digit (*flexores breves*), except the last, in which the fibular one is inseparable from the opponens; and (3) a *dorsal layer*, which, with the exception of the *abductors hallucis*, the *abductor indicis*, and the *abductor minimi digiti* is wanting, so far as regards the remaining dorsal interossei.

There is an *abductor ossis metatarsi minimi digiti*, and the little toe is furnished with an *opponens*. A more complete account of these muscles, both in the Koala and in the Opossum, with illustrations, will, however, be included in Dr D. J. Cunningham's report on the Marsupialia of the "Challenger" expedition.

#### ARTHROLOGY.

Although freely movable, owing to the laxity of its connections, the sterno-clavicular joint is amphiarthrodial in its structural features. There is no coraco-acromial ligament. The elbow-joint is only noteworthy because slight lateral and rotatory movements are permissible. Similar movements, more limited in extent, have recently been claimed for the corresponding joint in man.

Inferiorly, the radius and ulna do not articulate directly with each other, but are held loosely together by ligamentous fibres. A strong interosseous ligament stretches from the ulnar side of the lower extremity of the radius to the adjacent angle

<sup>1</sup> "On the so-called Movements of Pronation and Supination in the Hind-Limb of certain Marsupials," *Journ. of Anat. and Phys.* vol. xv. p. 392.

of the cuneiform bone; these bones, with the pisiform, move together during pronation and supination of the forearm, the movements taking place at an enarthrodial joint, the concavity of which is formed by the cuneiform and pisiform bones, the convexity by the rounded lower end of the ulna. A transversely elongated concave articular surface is constituted by the lower surfaces of radius and cuneiform bone, this receives the convex proximal surfaces of the scapho-lunar and unciform bones, and thus a joint is formed closely resembling the wrist-joint in man. It is at this joint that the usual antero-posterior and lateral angular movements of the wrist take place. An imperfect interarticular fibro-cartilage intervenes between the radius and scapho-lunar bone.

Referring to the lower extremity, I can confirm the observations of Mr Forbes<sup>1</sup> with respect to the existence of a ligamentum teres, the depression for which on the head of the femur I found in all cases distinctly marked. The main peculiarities of the articulations of the leg have been already referred to in a former communication.<sup>2</sup>

#### CONCLUDING REMARKS.

In a brief account of the visceral anatomy of the Koala, which appeared in the pages of a recent number of this *Journal*,<sup>3</sup> I had to explain that in consequence of some injury having happened to the pelvis and lower part of the abdomen of the only female specimen which came into my possession, I was not able to speak with absolute certainty as to the anatomy of the pelvic organs. The observations of Mr Forbes, which subsequently came under my notice, show, however, that my statements regarding the viscera in question were substantially correct. On other points, also, where the same organs are referred to, our observations practically coincide. The exceptional advantage of being able to examine a fresh specimen enabled Mr Forbes to give a valuable description of the brain of the Koala, and thus the anatomy of the soft parts of *Phascolarctos* may be regarded as fairly complete.

<sup>1</sup> "Some Points in the Anatomy of the Koala," *Proc. Zool. Soc.* 1881, p. 193.

<sup>2</sup> *Journ. Anat. and Phys.* vol. xv. p. 392.

<sup>3</sup> *Ibid.* vol. xv. p. 466.

Briefly, the leading peculiarities, so far as the visceral anatomy is concerned, may be thus summarised:—Well defined cheek pouches exist; the stomach, which is uncomplicated with sacculi, is provided with a special glandular apparatus; the cæcum is enormously long, and its mucous membrane, as well as that of the commencing colon, is longitudinally plicated; these portions of the alimentary canal may or may not be sacculated.<sup>1</sup> Two small glandular patches exist, one on each side of the ileo-cæcal aperture. The liver is noteworthy as presenting the maximum marsupial tendency to lobar subdivision, and as possessing a gall-bladder which projects in a striking manner beyond its ventral margin. A vesicula prostatica exists in the male, and the prostate gland, which is comparatively small, evidences a tendency to a division into two lobes. Mr Forbes's observations show further that, whilst the brain, heart and main vessels, and the lungs differ in no very important respects from these organs in other marsupials, there are still some points with reference to them which are worthy of special note as bearing on the affinities of the Koala, *e.g.*, the absence of an azygos lobe in the lungs, and the occasional independent origin of the left carotid artery from the aorta. From a general consideration of facts relating to the visceral anatomy, as well as from others referring more particularly to the skeleton, Mr Forbes considers the Koala to be so closely allied both to the Phalangers and to the Wombat, that he suggests the grouping of these together as subdivisions—*Phalangistinae*, *Phascolarctinae*, and *Phascolomyinae*—of the *Phalangistidae*.

Doubtless the classification of the three groups just mentioned would not be materially influenced by a comparison of their muscular systems. The modifications met with in the muscular anatomy of the Koala are, however, so remarkable that, without laying undue stress upon myological features as constituting generic distinctions, it will at least be interesting to see whether muscular arrangements similar to those described in *Phascolarctos* obtain throughout *Phalangistidae*.

<sup>1</sup> Mr Forbes (*loc. cit.* p. 184) refers to the "sacculation of the colon and cæcum, previously described by Mr Martin (*loc. cit.*, p. 111), but states "I could find no traces of any such sacculation in the fresh Koala examined by me." In all my specimens, which were spirit preserved, the sacculation was distinct and regular.



As the most important of these myological features, which, in any case, are of considerable anatomical interest, I would refer particularly to those arrangements which appear to indicate an adherence to a more primitive lamination of the muscles generally, *e.g.*, the continuity of the panniculus carnosus with the platysma, and the prolongation of the latter to ear, eye, and mouth; the prolongation of the sterno-hyoid and omo-hyoid muscles beyond the hyoid bone; the union of the styloid group of muscles into a single sheet; the inseparable character of the cervical subvertebral muscles; the undivided rhomboidei; the occasional continuation of the lowest fibres of the trapezius beyond the clavicle to the pectoral fascia.

Noteworthy, too, is the segmentation, by transverse tendinous intersections, of the internal oblique muscle, the absence of a distinct quadratus lumborum, and the arrangement of the hyoglossus. As regards the fore-limb, may be noted the extreme length of the supinator longus, the character of the coracobrachialis, the small pronator quadratus, the absence of an extensor secundi internodii, the arrangement of the flexor perforatus, and of the intrinsic muscles of the hand.

In the hind-limb, the absence of any iliac attachment of the gluteus externus, the single origin of the biceps, the separate character of the hamstrings, the occasional attachment of the gracilis to the marsupial bone, the absence of a soleus, the double tibialis posticus, the peronæi, the muscular modifications, associated with the so-called movements of pronation and supination of the leg, and the intrinsic muscles of the foot, constitute the chief points of interest.

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NOTE.—With the external characters of the Koala, naturalists have long been familiar, whilst anatomists are well acquainted with the principal points of its odontology and osteology. To none of these, therefore, has reference been made in this or any of my former communications on the anatomy of the Koala.

Diverse statements, however, as to the number of ribs possessed by the Koala obtain in different accounts of its osteology, and to these I would, before concluding, briefly refer. Professor Owen<sup>1</sup>

<sup>1</sup> Art. "Marsupialia," *Cyclop. Anal. and Phys.* vol. iii. p. 268.

affirms the existence of *thirteen* pair of ribs, the common number amongst marsupials. Professor Flower,<sup>1</sup> on the contrary, states that the marsupialia have nearly always thirteen pairs of ribs, the Koala (*Phascolarctos*) with but *eleven*, and the common Wombat (*Phascolomys*) with fifteen, being the only known exceptions. In two of my specimens *eleven* ribs only were present, whilst in one there were *twelve*, the latter, according to Professor Owen and Cuvier, being the number met with in the Petaurists.<sup>2</sup>

<sup>1</sup> *Introduction to the Osteology of the Mammalia*, p. 94.

<sup>2</sup> Art. "Marsupialia," *Cyclop. Anat. and Phys.* vol. iii. p. 277.

THE ACTION OF SALINE CATHARTICS. By MATTHEW HAY, M.D., *Demonstrator of Practical Materia Medica, in the University of Edinburgh.*<sup>1</sup>

OF the various classes of cathartics in common use the saline have been the most recently introduced. Common salt, it is true, was occasionally employed as a purgative by the ancients, and probably the tartrates of potash were similarly used by Paracelsus and his followers; but it was not until the discovery of sulphate of soda by Glauber in 1658, named by him *sal mirabile*, that the attention of physicians was drawn to this class of cathartics. Five years later, the Duke of Holstein paid 500 thalers for the secret of the preparation of the long famous *sal polychrestus* or *tartarum vitriolatum*, a mixture probably of the neutral and the acid sulphates of potash. Seignette, an apothecary of Rochelle, prepared, in 1672, the double tartrate of potash and soda. Grew, in 1675, was the first to observe the presence of a purgative salt in the springs at Epsom, a salt which was afterwards shown by Dr. Black to be the sulphate of magnesia. Phosphate of soda was, in 1737, found in the urine by Hellot, and, some fifty years later, it was introduced into medicine as a purgative by Dr. Pearson. Thus, by the commencement of the present century, all the principal cathartic salts now in use had been discovered.

In a few of the works on the materia medica published towards the close of the last century, as those of Cullen and of Lewis, some attempt was made to indicate the manner in which the salts produced purgation; but, in the absence of experiment, and with a very imperfect knowledge of alimentary physiology, the opinions expressed are of little or no value. The first scientific explanation of their action was based upon the discoveries of Fischer and of Dutrochet,<sup>2</sup> who had demonstrated the remarkable physical property of salts known as osmosis. Poisseuille,<sup>3</sup> in a very ingenious monograph, believed to have found

<sup>1</sup> This paper is the substance of a thesis, presented on the 1st of May 1881, for graduation as doctor of medicine in the University of Edinburgh, and to which a gold medal and the Goodair memorial prize were awarded.

<sup>2</sup> Dutrochet, *Recherches sur l'endosmose et l'exosmose*, Paris, 1828.

<sup>3</sup> Poisseuille, *Recherch. expériment sur les mouvements des liquides dans les tubes de petites diamètres*, Paris, 1828. *Comptes rendus*, t. xix. 1844, p. 24.

in this and other physical qualities of salts, and indeed of all drugs, a complete explanation of their action. That the purgative effect of salts was due to their endosmotic power was the most striking, and appeared the most satisfactory, of his numerous deductions. This view had been independently arrived at by Liebig,<sup>1</sup> but not stated with much certainty by him until 1848. Both had observed the osmotic action of salts in relation to blood-serum and to water, but neither had controlled their experiments by any on the living body. With what degree of care these observers, especially Poesseuille, arrived at this conclusion, may be gathered from the fact that, paying no heed to the irritant or specific action a purgative may possess, and absolutely regardless of what was long before known, that certain of the vegetable cathartics can act as efficiently when introduced into the blood as when administered by the mouth, Poesseuille ascribed the purgative action of even senna, colocynth, aloes, scammony, jalap, and castor oil to their endosmotic power, an application manifestly absurd.

Many other observers, working with much the same method and materials, signified their adherence to the view of Liebig and Poesseuille, in so far as it concerned the action of saline cathartics. The most distinguished among these was Mateucci,<sup>2</sup> who, in conjunction with Cima, has furnished physiological physics with some extremely ingenious and elegant experiments. Otto Funke,<sup>3</sup> and his pupil Krug,<sup>4</sup> from investigations with saline fluids and animal membranes, arrived at the same conclusion; and, much more recently, Rabuteau<sup>5</sup> has endorsed the opinion of these physiologists, since he has observed that the injection of a large dose of sulphate of soda into the circulation of a dog is followed by constipation rather than by purgation, while if the same dose be administered by the mouth purgation will ensue, showing that on whichever side of the intestinal mucous membrane the salt be placed there is an endosmotic flow of fluid towards it. Still more recently, Heidenhain,<sup>6</sup> from not having perceived the same effect follow the action of a solution of sulphate of magnesia on the glandular structure of the colon as when pilocarpin, a glandular stimulant, was introduced, has concluded that the salt causes "a simple endosmotic capillary transudation, and not a real glandular secretion."

Claude Bernard<sup>7</sup> was among the first to check the early progress of this too physical school. He strongly objects to the experiments and conclusions of Poesseuille, as making no allowance for the action of the

<sup>1</sup> Liebig, *Untersuch. üb. die Mineralquellen zu Soden u. Bemerk. üb. die Wirkung der Salze auf den Organismus*, Wiesbaden, 1839. *Ueber die Saftbewegung*, 1848.

<sup>2</sup> Mateucci, *Lectures on the Physical Phenomena of Living Beings*, Translated by Pereira, 1847, p. 73.

<sup>3</sup> Funke's *Physiologie*, Leipzig, 1876, S. 237.

<sup>4</sup> Krug, *Nonnulla de theoria endosmos. etc. Dissert.* Lipsiæ, 1859.

<sup>5</sup> Rabuteau, *Mémoires de la soc. d. biologie*, 1868, p. 21. *L'union méd.* 1871, Nos. 50 et 52. *Gaz. méd. de Paris*, 1879, 14 June.

<sup>6</sup> Heidenhain, *Physiologie d. Absonderungsvorgänge*, Hermann's *Hilbuch. d. Physiologie*, 1880, Bd. v. S. 172.

<sup>7</sup> Claude Bernard, *Substances toxiques et médicamentieuses*, 1857, p. 69, et seq.

nervous system and the mass of complex phenomena which constitute the living individual—"Partout où il existe de la matière, cette matière est soumise sans doute aux lois générales de la physique et de la chimie ; mais, chez les êtres vivants, l'action de ces lois est étroitement liée à une foule d'autres influences qu'on ne saurait nier." Sugar, he says, whose endosmotic power is very high, should, according to the experiments of Poesseuille, act pre-eminently as a purgative. Bernard's criticism is, so far, thoroughly sound and justifiable ; but when he proceeds to state, as an objection to Poesseuille's theory, that sulphate of soda purges when directly introduced into the veins, and even better than when swallowed, he makes an assertion which nearly all subsequent observers have failed to prove. As he quotes no experiments in its support, it is probable that he trusted to the statements of others, or to a common, but, as will be afterwards more fully shown, an erroneous belief. This inaccuracy does not, however, impair the correctness of his conclusions ; he was seeking to establish general principles of medicinal action, not to discuss in particular the mode of action of a saline purgative.

The theory of the endosmotic action of the salt was more powerfully opposed by the experimental investigations of Aubert<sup>1</sup> and of Buchheim<sup>2</sup> and his pupil Wagner,<sup>3</sup> who clearly demonstrated that this theory did not offer a complete and satisfactory explanation of the purgative action.

Aubert, under the direction of Jolly, whose memoir on endosmotic equivalents has become classical, investigated the osmotic relation of various salts, diuretic as well as purgative, to blood-serum, with the result, that he ascertained that the salt with the highest endosmotic power is not the most purgative, as would have been expected were Poesseuille and Liebig's theory correct. Comparing three of the most important purgative salts, it was found by Aubert that phosphate of soda possessed an endosmotic power considerably more than twice as great as that of sulphate of soda, while the endosmotic equivalent of the latter salt was nearly thrice that of sulphate of magnesia ; yet sulphate of magnesia is probably the most powerful cathartic of the three, and the phosphate is certainly the weakest. The inference is unavoidable that the endosmotic power of a salt is no indication, or, at any rate, not the sole measure, of its purgative strength.

From a few other experiments, Aubert concludes that the degree of concentration of the salt solution administered does not influence the degree of purgation, a point which I shall afterwards have to dispute. On the whole, he is inclined to ascribe the catharsis mainly to increased peristalsis, aided to some extent by the osmotic action of the salt.

Three years later, Buchheim, one of the greatest of modern pharmacologists, and whose death we had recently to deplore, conducted a series of experiments on himself and his pupil Wagner. In several instances they estimated the quantity of the purgative salt recoverable

<sup>1</sup> Aubert, *Zeitschrift f. rationelle Medicin*. Bd. i. 1851, S. 93, u. 225.

<sup>2</sup> Buchheim, *Arch. f. physiolog. Heilkunde*, 1854, S. 93.

<sup>3</sup> Wagner, *De effectu natri sulfurici*, *Dissert. inaug.* Dorpat, 1853.

from the urine after its administration in varying doses and degrees of dilution. They arrive at much the same conclusions as Aubert, and attribute to increased peristalsis—for which they did not, any more than Aubert, furnish direct proof—the principal action in purgation. They make the further suggestion, but equally fail to prove it, that the water of the stools is not the result of increased secretion from the intestinal mucous membrane, but rather the water in which the salt was dissolved when administered, or with which it came in contact in the alimentary canal. Their own experiments ought to have led them to an opposite conclusion; for on no occasion did they find that the concentration of the solution of the salt affected the activity of the salt. In contradiction of a single and unsatisfactory experiment of Aubert, they observed no purgation follow the injection of the salt into the circulation. In this they were supported by some experiments made in the same year by Donders.<sup>1</sup>

A few years previous to the investigations of these observers, the eminent chemist and physiologist, Carl Schmidt,<sup>2</sup> had advanced a new theory, or rather revived the theory of many of the older pharmacologists, that the salt excited, from the effect of local stimulation, a "transudation" through the capillaries of the intestine, which was neither an intestinal secretion nor an inflammatory exudation. He based this view upon the resemblance in chemical composition of the purgative stool to the choleraic dejection, the latter of which he believed to be produced in a similar manner. This theory has not met with much support, Radziejewski<sup>3</sup> and others having proved that it is untenable. By some, however, and even by so high an authority as Hoppe-Seyler,<sup>4</sup> and by those generally who do not believe that the existence of an intestinal secretion has been sufficiently demonstrated, it is still regarded as the most probable of the various theories advanced, as Schmidt's analysis of the purgative dejection agrees very closely with that of the serous transudation of dropsy.

To Headland,<sup>5</sup> at this time, are we indebted for a still more novel view of the action of the saline cathartic, a view which has been entirely ignored by all subsequent investigators. In his well-known work on the Action of Medicines, he had formulated a law, that all medicines pass into the circulation before they can exercise their action on the body; and, as a particular instance and proof of this, referred to the purgative action of certain salts. Misled by Aubert's paper, he wrongly assumed that these salts purge when injected into the blood, and unwarrantably deduced that only after absorption into the blood, when administered in the usual way, could they purge, the salts being absorbed by the stomach and upper part of the small intestine, and in the process of their elimination by the glands of the lower part of the

<sup>1</sup> Donders, *Physiologie d. Menschen*. Leipzig, 1859, Bd. i. s. 314.

<sup>2</sup> Carl Schmidt, *Characteristik d. epidem. Cholera*, Leipzig, 1850, s. 90, *et seq.*

<sup>3</sup> Radziejewski, "Zur physiolog. Wirkung d. Abführmittel," *Reich. u. Du Bois-Reymond's Archiv*. 1870, Hft. i. s. 30, u. 67.

<sup>4</sup> Hoppe-Seyler, *Physiolog. Chemie*, 1878, Th. ii. s. 275, u. 358.

<sup>5</sup> Headland, *Action of Medicines*, 4th ed. 1867, p. 441, and p. 55, *et seq.*

small intestine exciting the secretion of these glands. This belief he founded upon three experiments on as many dogs, to each of which he administered a purgative dose (180 grains) of sulphate of magnesia. One he killed three-quarters of an hour afterwards; another in an hour and a half; and the third after two hours. By a somewhat imperfect method of analysis, the amount of the magnesia recoverable from the contents of the alimentary canal of each dog was estimated, and the corresponding quantity of sulphate of magnesia calculated. From the first dog he obtained fifty-six grains of the salt; from the second, seventy-seven grains; and from the third, ninety-seven grains. The conclusion appears unavoidable, that the salt is first absorbed and afterwards excreted. These experiments stand in opposition to the observations of all investigators since Headland's time, a single experiment by Carpenter<sup>1</sup> alone appearing to support them, purging having been produced when sulphate of magnesia was injected into the stomach with its pylorus ligatured. How far these observers are right, a large number of experiments of my own will determine.

In the same year in which Buchheim published his paper, Colin<sup>2</sup> made known a new method for ascertaining the action of a purgative salt on the intestine, a method which had been previously employed by Hunter, Magendie and others in the study of absorption, but not in determining the effect of purgatives. Moreau<sup>3</sup> did not make use of this method until many years afterwards, although to him is generally and wrongly ascribed its first employment. It consisted in making an incision into the abdomen of the horse (Colin) or dog (Moreau), exposing the intestine, into a loop of which, separated from the remainder of the gut by a couple of ligatures, was injected a strong solution of the purgative salt. As controls, a loop was ligatured off at each end of the original loop, and in these no salt solution was placed. In every instance they found that the salt solution had largely increased in bulk, while the adjacent controls remained perfectly empty. These two observers do not, however, agree as to the nature of the secretion. Colin regards it as a serous exudation; while Moreau maintains that it is a true *succus entericus*. The latter is of this opinion, because the fluid closely resembles that obtained by division of the mesenteric nerves supplying the loop, an experiment for which we are indebted to the ingenuity of Moreau.<sup>4</sup> Both fluids did not contain more than from 0.1 to 0.2 per cent. of albumen, a quantity much smaller than is met with in serous transudations. As many physiologists still doubt the existence of a true *succus entericus*, and as the proof of its existence and the capability of its rapid secretion is of the highest importance in solving the nature of purgative action, it is well to consider upon what grounds this fluid obtained by section of the mesenteric nerves can be regarded as a veritable intestinal juice, and not a mere serous transu-

<sup>1</sup> Carpenter's *Comparative Physiology*, 4th ed.

<sup>2</sup> Colin, *Physiologie comparée*, 1854, t. i. p. 649.

<sup>3</sup> Moreau, *Archiv. général. d. médecine*, VI. série, t. xvi. p. 234, 1870.

<sup>4</sup> Moreau, *Comptes rendus*, t. lxvi. p. 554; confirmed by Asp, *Ludwig's Archiv. 1868*; and Radziejewski, *op cit.* S. 41.

dation. Its chemical composition, as Moreau has pointed out, clearly supports this belief. Unfortunately, Moreau has not furnished us with an examination of its digestive properties. What these are will again be discussed. In the meantime, I would advance this further argument in favour of the fluid being a true secretion; that in the case of certain well-known secreting glands, as the submaxillary and sublingual salivary glands, and the pancreas, Bernard,<sup>1</sup> Heidenham,<sup>2</sup> and Bernstein<sup>3</sup> have proved that a so-called "paralytic" but true secretion is produced by the division of all the nerves supplying these organs; why not from the intestinal follicles after division of the mesenteric nerves?

These experiments with purgatives, after the method of Colin and Moreau, have since been repeated with extreme care by Vulpian,<sup>4</sup> and by Lauder Brunton,<sup>5</sup> by Böttger<sup>6</sup> in the laboratory of Bernstein, and by Brieger<sup>7</sup> at the suggestion and under the guidance of Cohnheim, and invariably with the same result—the excitation of a profuse secretion in the injected loop.

Lauder Brunton's experiments were professedly a careful repetition on the cat of those of Moreau and Vulpian, for the purpose of testing the accuracy of their results. He states his belief that the fluid secreted, on account of its containing little albumen, is a true intestinal secretion, and not a transudation.

Böttger, in his investigation, used frogs and rabbits. The secretion, which was copious, was, as obtained from the latter animals, mixed with blood, and yielded a large precipitate of albumen. He believed that the results of his experiments were best explained by the theory of Carl Schmidt, as the secretion possessed the composition of a transudation. He apparently overlooked the fact that blood is not usually present in the purgative stools of sulphate of soda, and that its presence in the fluid he obtained would sufficiently account for the large quantity of albumen.

Brieger improved upon the method of the others to the extent, that he washed out the loop of intestine before injecting the saline; so that the secreted fluid might be free from mixture with the remnants of partially digested food which were possibly present. The secretion obtained was yellowish in colour, and capable of digesting starch, and of dissolving raw fibrin, but was without action on boiled fibrin; it contained mucous and epithelial corpuscles, but no hæmocytes. The mucous membrane was pale and uncongested. The fluid, he concludes, is a true intestinal secretion.

<sup>1</sup> Bernard, *Leçons sur les propriétés physiologiques des liquides de l'organisme*, Paris, 1859.

<sup>2</sup> Heidenham, *Stud. d. physiolog. Instit. zu Breslau*, Leipzig, 4 Hft.

<sup>3</sup> Bernstein, *Sitzungsab. d. Akad. d. Wiss. zu Leipzig*, 1869, S. 96.

<sup>4</sup> Vulpian, *Gazette médicale*, 1873, p. 300. *Leçons sur l'appareil vasomoteur*, t. i. p. 458, et seq.

<sup>5</sup> Lauder Brunton, *Practitioner*, vol. xii. 1874, pp. 342 and 403.

<sup>6</sup> Böttger, *Ueber d. physiolog. Wirkung d. Abführmittel*. Inaug. dissert. Halle, 1874.

<sup>7</sup> Brieger, *Arch. f. experim. Path. u. Pharm.* Bd. viii. 1878, S. 355.



Vulpian's investigation is one of greater length and complexity than those of the others, and requires a more extended notice, the more as he furnishes a new explanation of the action of the salt. He asserts that the salt neither excites a follicular secretion nor a dropsical transudation, but irritates the intestinal mucous membrane, causing an inflammatory exudation. The irritation, he believes, is not produced directly, but reflexly through a nervous mechanism involving the sensory nerves of the intestine, the various ganglia of the plexuses of Meissner and Auerbach, the solar and mesenteric plexuses, and even those of the lower thoracic nerves, and, finally, the vasomotor nerves. In opposition to this theory of Vulpian, I shall so far anticipate the results of my own experiments as to deny that the salt ever excites an inflammatory or catarrhal irritation of the intestinal mucous membrane, as judged from an examination of the membrane after it has been in contact with a solution of the salt, even so strong as twenty per cent.; and, as regards the involvement of a reflex nervous mechanism, it may be true, but it is quite devoid of experimental proof, if we except the experiment of Moreau, in which he showed the effect of the division of the mesenteric nerves on the intestinal secretion. Although the secretion is produced in this case by interference with the nerve supply, it does not necessarily follow that secretion cannot be excited by direct stimulation of the intestinal glands by the salt. Vulpian asserts that the fluid obtained by Moreau in this experiment is not intestinal juice, as it has not been shown to possess any digestive power, and is more fluid than the true juice. He apparently forgets that Radziejewski<sup>1</sup> found that it digested starch with ease, and dissolved albumen; and I do not know that more has been satisfactorily proved for the *succus entericus*. And, as to its fluidity, ordinary methods have failed to separate the juice in sufficient quantity and purity to enable us to form an opinion of the constancy and value of this character.

Besides experiments with purgatives made by the method of Colin and Moreau, Vulpian, through an incision in the abdominal wall, injected into the duodenum of the unligatured intestine of a dog a quantity of a solution of the saline; and an hour afterwards extended the incision upwards to the xiphoid cartilage and downwards to the symphysis, and observed the appearance and movements of the intestine, as the salt solution passed towards the colon. On first exposing the gut, he noticed that the duodenum was reddened, and that gradually and slowly the jejunum and ileum became congested. This congestion he attributed to the action of the salt, but it was obviously due to the exposure of the delicate peritoneal surface of the intestine to the cold and dry atmosphere. Many investigators,<sup>2</sup> as well as myself, have had occasion to observe how quickly an inflammatory reddening suffuses the walls of the gut, when it is exposed to the ordinary atmosphere; one or two minutes are often sufficient. The conditions of this experiment are not good, and the results, therefore, untrustworthy.

<sup>1</sup> Radziejewski, *op. cit.* S. 45.

<sup>2</sup> Legros et Onimus, *Journ. de l'anatomie et physiolog.* 1869, p. 178. Houckgeest, *Pflüger's Archiv.*, 1872, S. 266.

Vulpian also injected the sulphate of soda into the circulation, and obtained the same negative result as Buchheim, Wagner, Donders, and Rabuteau. In this he also agreed with Jolyet and Cahours,<sup>1</sup> who found from two experiments, likewise made on dogs, that neither the sulphate of soda nor the sulphate of magnesia purge when so injected. That is their conclusion, although they record that two liquid bilious stools were evacuated during the following night by the dog which received the latter salt.

In the course of his investigation, Vulpian, having become aware of some experiments by Luton,<sup>2</sup> in which the subcutaneous injection of small doses (1 decigram.) of sulphate of magnesia in man was followed by purgation, repeated these experiments on dogs, and with a like result. He had previously ascertained that, curiously enough, a large dose (10 grms.) of sulphate of soda did not purge when so injected. Premising that the small dose is absorbed, and thereby produces its purgative effect, while forgetting that a similar dose injected directly into the blood will not purge, he proceeds to explain the negative result following the subcutaneous injection of the large dose as being due to its non-absorption, from the amount of inflammatory irritation and effusion it excites in the surrounding tissues; surely a most fallacious assumption, as at least out of 10 grms., 1 decigram., or one hundredth part, must be absorbed and pass into the circulation.

In the following year, Carville,<sup>3</sup> at Vulpian's suggestion, repeated these experiments on dogs, and, having injected subcutaneously a decigram. of sulphate of magnesia, he killed the dogs before purgation had occurred. In all he found a catarrh of the whole intestine, the mucous membrane being congested and covered with a *sanguineous* effusion. This Vulpian supposes to thoroughly harmonise with his theory of inflammatory irritation; but, it is certainly remarkable that, while the absorption of a decigram. will so greatly inflame the intestine, other investigators have never observed this condition follow the introduction of the salt in very much larger quantity into the alimentary canal, or its direct injection into a loop of the intestine.

Aubert, Buchheim, and Wagner I have already referred to, as assigning to excited peristalsis of the intestines the most important part in the production of saline catharsis. This is an opinion still very largely shared by German pharmacologists, especially since it has received apparent confirmation in Thiry's<sup>4</sup> well-known communication, and in the laborious investigation of Radziejewski.<sup>5</sup>

Thiry, whose work was carried out in the laboratory of Ludwig, employed a new and ingenious method for isolating a portion of the gut. He made an incision through the abdominal wall of a dog, and withdrew a loop of the small intestine, which, by dividing at its two

<sup>1</sup> Jolyet et Cahours, *Archives de physiologie*, 1869, p. 113.

<sup>2</sup> Luton, *Bulletin de la société méd. de Reims*, 1873, p. 126. *Gaz. hebdom.* 1874, p. 455.

<sup>3</sup> Carville, *Gaz. hebdom.* 1874, p. 405.

<sup>4</sup> Thiry, *Sitzungsb. d. k.k. Akad. d. Wissenschaft.*, Sitz. 25 Febr. 1864. S. 19.

<sup>5</sup> Radziejewski, *op. cit.*

extremities, he completely separated from the remainder of the intestine, keeping, however, its mesentery intact. The continuity of the alimentary canal he restored by carefully joining with sutures the cut ends of the intestine. The isolated loop was then carefully closed by sutures at its one extremity, which was replaced within the abdomen, while the other extremity, kept patent, was sewn to the margins of the ventral wound. In those dogs, in which the operation succeeded, Thiry thus contrived to have a portion of the intestine free from contact with the aliment, and communicating with the outer air by an abdominal fistula, accordingly, ready of access and easy of observation. No experiment was made until at least fourteen days afterwards, which time was required for the healing of the wounds.

Thiry's main object was to examine the nature of the *succus entericus*, which he hoped to have obtained abundantly by this method. In this he was disappointed; for the strongest mechanical irritation of the mucous membrane of the isolated loop did not yield him more than 4 grms. of secretion per hour, from a loop 15 centimetres long. The injection of a strong solution of sulphate of magnesia equally failed to excite secretion. Thiry believed, from the latter experiment, to have furnished proof of Aubert's theory, that the salt does not stimulate the intestinal secretion, but acts by increasing the peristalsis.

There is, however, this serious objection to Thiry's method, that functional disuse of the fistulous loop for fourteen days or longer with the concomitant severe disturbance produced by the operation, in all probability, seriously impaired the secretory activity of the Lieberkühnian follicles. Even the few grammes of fluid obtained by mechanical irritation possess, according to Hoppe-Seyler,<sup>1</sup> exactly the characters of serum, and are, therefore, an inflammatory exudation, and not a true secretion. Schiff,<sup>2</sup> Leube,<sup>3</sup> Quinke,<sup>4</sup> Paschutin,<sup>5</sup> and Radziejewski<sup>6</sup> have repeated these experiments, and in every instance with a similar result. The last of these observers also administered a purgative dose of the salt *per os*, and collected the fluid which dropped from the fistula during the succeeding twelve hours. He found that, although free purgation had meanwhile occurred, there was no increase in the secretion from the isolated loop. If Thiry's method were trustworthy, Radziejewski's experiment would be opposed to the theory of Headland.<sup>7</sup>

Radziejewski conducted his investigation in the laboratories of Kühne and of Du Bois-Reymond, to the former of whom and Rosenthal he specially acknowledges his indebtedness for suggestions and assistance. As a result of his labours he has furnished us with the most comprehensive contribution that has as yet been made to this

<sup>1</sup> Hoppe-Seyler, *op. cit.* S. 275, u. 360.

<sup>2</sup> Schiff, *Nuove ricerche sul potere digerente*, &c. Morgagni, July 1867, p. 5.

<sup>3</sup> Leube, *Jahresb. d. ges. Med.* 1868, S. 97.

<sup>4</sup> Quinke, *Archiv. f. Anatom. u. Physiolog.* 1868, S. 150.

<sup>5</sup> Paschutin, *Reichert u. Du Bois-Reymond's Archiv.* 1871, S. 305.

<sup>6</sup> Radziejewski, *op. cit.* S. 49.

<sup>7</sup> Headland, *supra*, p. 246.

discussion. It opens with a series of analyses, chiefly qualitative, of the fæces of the dog under normal conditions, and with a biliary fistula. He then proceeds to an examination of its composition after the administration of various purgatives, among others, sulphate of magnesia. Here he remarks that, with the exception of the increased proportion of water and the absence of indol, the stools are perfectly normal in composition, the albumen barely exceeding its usual quantity. From these facts and others he opposes strongly the theory of Schmidt—that the increase of fluid is due to serous transudation. The concluding sentence of his paper expresses this very distinctly—"die Entleerungen nach Abführmitteln sind Darminhalt, nicht Transsudat." Working with the object of proving that increased intestinal peristalsis is the principal factor, he frankly admits that his experiments, so far, do not positively prove it; for he expected to have found, had peristalsis been accelerated, that the fæces would have contained more partially digested food than they actually did.

The second part of his paper is occupied with an amplified repetition of Moreau's experiments on the effect of the division of the mesenteric nerves. In this he is not altogether very fortunate, for, in his first experiment, no secretion took place, although he had divided all the nerves entering the loop. A more careful examination at the autopsy revealed a few fibrils uncut. This is an experience which afterwards befell Vulpian,<sup>1</sup> and is of much interest and, I believe, importance, although neither observer draws any conclusion from it. Why does the copious secretion, following the division of the mesenteric nerves supplying a loop of intestine, not occur when a fibril or two escape section? The probable explanation seems to me to be that the mesenteric nerves not only inhibit secretion, but also excite absorption; and the latter process is, in all likelihood, much more active than the former, so that the small part of the mucous membrane of the loop still retaining its mesenteric nerve-supply is capable of absorbing all the fluid that the remainder of the mucous surface can secrete. That nerves do exercise a considerable influence over absorption, the beautiful experiments of Goltz<sup>2</sup> have demonstrated; and why not in the intestine, where, *par excellence*, we would expect this influence to be manifest? The introduction of this view, according to which the quantity of the intestinal fluids is governed by two different sets of nerves,—those of secretion, and those of absorption, may seem somewhat out of place; but, as will afterwards be seen, it is of importance in considering the action of a purgative. For the salt may inhibit absorption as well as excite secretion, and thus doubly aid the accumulation of fluid within the intestines.

In another experiment Radziejewski injected croton oil into a ligatured loop with its nerves undivided, and observed violent diarrhœa follow. This he believes to be due to reflex excitement of the peristalsis of the whole intestine, occasioned by the croton oil confined in

<sup>1</sup> Vulpian, *Leçons sur l'appareil vasomoteur*, t. i.

<sup>2</sup> Goltz, *Pflüger's Archiv*. Bd. v. S. 53.

the ligatured loop, forgetting, as his own protocol states, that the mucous membrane of nearly the entire length of the intestine was much congested, and the barrel above the ligatured coil distended with fluid, obviously an excited secretion. For neither of these conditions, of hyperæmia and hyper-secretion, could increased peristalsis account. Radziejewski was attempting to prove that excited peristalsis is the cause of all purgative action, and, in his eagerness to conform the results of his experiments to his theory, he clearly oversteps the limits of fair deduction. He did not inject a solution of a purgative salt into such a loop, and therefore omitted the experiment which operates most strongly against his theory.

Radziejewski closed his investigation with an examination of the time required by the food, with and without a purgative, to pass from the stomach to the cæcum, the latter having been rendered accessible by means of a fistula. Berries and crushed bones administered with sulphate of magnesia appeared at the cæcal fistula in much the same time as without the salt, but were preceded by a discharge of fluid. As a fact of practical value, he further observed that, especially after the administration of a drastic purgative, gastric digestion was completely arrested; pieces of meat which had been eaten along with the purge some hours previously, were taken from the cæcum much in the same raw state as when ingested. The last of his numerous experiments consisted in watching the effect produced by the purge on the peristaltic movements; but he never could distinguish more than a moderate movement (*mässiger Bewegung*).

The researches of Thiry and Radziejewski present the main facts on which those who believe that purgatives act by accelerating intestinal movement base their opinion. I have given a perfectly unprejudiced account of the investigations of these observers; and I think it will be readily granted that such a conclusion is not warranted by the results of their experiments. Moreover, others as Legros<sup>1</sup> and Onimus, and Van Braam Houckgeest,<sup>2</sup> with suitable apparatus for observing and registering the movements of the intestines, and using certain precautions to avoid exposure of the intestines to the cold and dry atmosphere, have made a particular study of the effect of purgative and other salts on these movements, and are quite unanimous in their conclusions that the saline cathartic does not increase the intestinal peristalsis.

Among the most recent contributions to the subject of purgative action are those of Aguilhon,<sup>3</sup> and Laborde,<sup>4</sup> and a further communication by Rabuteau.<sup>5</sup> Aguilhon, in a study of the action of the purgative waters of Châtelguyon, has concluded that the active ingredient is chloride of magnesium; and this salt he has found by experiment to purge, whether injected into the stomach or into the blood. Laborde

<sup>1</sup> Legros et Onimus, *Journ. de l'anat. et physiol. de Robin*, 1869, p. 187.

<sup>2</sup> Houckgeest, *Pflüger's Archiv* 1872, S. 266.

<sup>3</sup> Aguilhon, *Gaz. hebdom.* 1879, p. 336.

<sup>4</sup> Laborde, *Gaz. hebdom.* 1879, p. 352.

<sup>5</sup> Rabuteau, *Gaz. méd.* 1879, No. 29.

has observed increased peristaltic movements follow the intravenous injection of the salt, but in no case purgation. Rabuteau denies that it is capable of producing either effect when so injected.

Professor Rutherford<sup>1</sup> has lately supplied us with the results of an exhaustive research on the action of a large number of substances on the secretion of the bile, and has shown that whatever effect saline purgatives may exert on the expulsion of bile from the gall-bladder, they do not greatly influence the bile secretion. Phosphate of soda, sulphate of soda, sulphate of potash, and Rochelle salt all slightly increased the bile secretion, whilst sulphate of magnesia diminished it. Generally, he has found that even in the case of powerful cholagogues, as podophyllin, the greater the purgative action, the less is the excitation of the biliary secretion. The bile does not, therefore, appear to be an important source of the fluid found in the intestines during the action of a purgative. Rutherford also observed the action on the intestines of the various cathartics after their injection into the duodenum. In the case of the purgative salts, his solutions were so concentrated, rarely less than fifty per cent., that they produced, as the autopsy revealed, considerable inflammatory irritation of the intestinal mucous membrane, a condition which does not follow the injection of a weaker solution, as I shall have occasion to point out. The intestinal fluid he obtained must, accordingly, have been largely mixed with an inflammatory exudation; and it is impossible to ascertain to what extent the increase of the intestinal fluid which he observed to follow the injection of the salt, was due to the presence of this exudation.

To complete the literature of this subject, I have yet to mention that Professor H. C. Wood<sup>2</sup> of Philadelphia has made the statement that section of the vagi prevents the action of the purgative, and ascribes this effect to some vaso-motor disturbance, believing that the vagi may antagonise the splanchnics, whose division is generally considered to be followed by dilatation of the blood-vessels of the intestine.

*En resumé*, it will be gathered from this sketch of the many attempts to solve the nature of saline purgation, that pharmacologists are very far from being agreed as to the nature of that action. There are some who maintain that the salt excites a flow of fluid into the alimentary canal, others who as confidently assert that it merely excites peristalsis.

Of the former, Liebig and Poisseuille, without venturing to speculate as to the exact nature of the fluid, believe that it is extracted from the blood through the capillaries of the intestine in virtue of the endosmotic power of the salt. Colin, Moreau, Lauder Brunton, Böttger, Brieger, and Rutherford conclude that

<sup>1</sup> Rutherford, *Action of Drugs on the Secretion of the Bile*, 1880.

<sup>2</sup> Wood, *American Journ. of Med. Sciences*, 1870, p. 395.

the fluid is probably intestinal juice, secreted under the local stimulus of the salt. Vulpian, on the other hand, regards it as an inflammatory exudation; while Schmidt calls it a transudation produced in the same manner as the choleraic discharge. Headland thinks that the salt, after being absorbed by the blood, stimulates Lieberkühn's glands in the process of its excretion. If this be so, why does the salt not purge when injected into the blood, as Buchheim, Donders, Rabuteau, and others have shown?

Of the latter, Thiry and Radziejewski are of opinion that the salt produces its effect by mere excitation of peristalsis; whilst Aubert, and Buchheim, and Wagner consider that the increased peristalsis is aided by the slow diffusibility or absorbability of the salt, and by the salt combining with the water of the alimentary canal to hinder its absorption. But Legros and Onimus, and Houckgeest could not perceive that the movements of the intestine were to the smallest extent stimulated by the presence of the salt.

Finally, the remarkable results obtained by Luton and Carville require to be reconciled with the experiments of the other investigators.

With the prevalence of so many theories and so much uncertainty as to the action of so commonly employed pharmacological substances as saline cathartics, there is ample justification for another effort being made to offer a more comprehensive and satisfactory explanation of their action. There is the further inducement to their study, that their action presents the key to the action of nearly all other purgatives, and to many common physiological and pathological phenomena. "If the action of sulphate of soda were known," wrote Buchheim at the conclusion of his paper in 1854, "it would help us to ascertain with greater precision the influence of other remedies on the body, for example, as a proof-stone for the remedies used to cure diarrhœa."<sup>1</sup> And after twenty years, by no means unfruitful in investigations of this action, Vulpian cannot more fitly introduce its discussion into his lectures than with the words, "Tout le monde parle de l'action des purgatifs, et, cependant, s'il y a une action encore peu connue, c'est bien celle de médicaments."<sup>2</sup> This, after the able

<sup>1</sup> Buchheim, *op. cit.*

<sup>2</sup> Vulpian, *Leçons cit.*

investigations of Buchheim, Thiry, Moreau, and Radziejewski, is certainly no compliment to pharmacology. But the difficulties which surround such an investigation may well be the excuse for the paucity of its results. The very complex, and as yet very imperfectly understood nervous supply of the intestines, and the disturbance produced in experiments as those of Colin and Moreau by the mere exposure of the sensitive abdominal viscera, and, generally, the very unsatisfactory condition of our knowledge of the physiology of the intestines, have prevented previous observers from employing good methods of experimentation, and from drawing unassailable conclusions from their experiments. By repeating some of these experiments under new and varied conditions, I have endeavoured to estimate the true value of their methods, and by the adoption of new methods in which the normal state of the alimentary canal has been undisturbed by surgical operation, I have been enabled to obtain results, to which little or no objection can be offered. These I shall present in the form of several series of experiments, of which the following is the probable order, an order which, if in some respects more arbitrary than natural, approximates to that in which the experiments were made:—

*Series, A.*—The effect on purgation of the administration of the salt by the mouth in a state of concentration and of dilution; and the elimination of the salt by the kidneys and the alimentary canal.

*Series, B.*—The effect of the salt on the intestine when injected directly into the viscus, after the method of Colin and Moreau, with analyses of the fluid as to the salt it contained, and as to its digestive and other properties.

*Series, C.*—The effect of saline purgation on the concentration of the blood.

*Series, D.*—The effect of the salt on the alimentary canal, and its absorption from the canal, as ascertained by killing the animals at stated intervals after the administration of the salt by the mouth, and measuring the fluid in the canal, and estimating the quantity of the salt present.

*Series, E.*—Its purgative effect when injected into the blood.

*Series, F.*—Its purgative effect when injected subcutaneously.



*Series, G.*—Its effect on the circulation.

*Series, H.*—Its effect on the secretion of urine.

#### SERIES OF EXPERIMENTS, A.

The effect on purgation of the administration of the salt by the mouth in a state of concentration and of dilution; and the elimination of the salt by the kidneys and the alimentary canal.

Assuming that the method of Colin and Moreau<sup>1</sup> is open to objection on account of the violence done to the natural conditions of the intestines, and that the results they obtained in proof of a salt exciting secretion may be fallacious, and granting that Legros and Onimus, and Houckgeest<sup>2</sup> have succeeded in demonstrating that the purgative does not stimulate peristalsis, it is possible that the salt may produce purgation by merely uniting with the water with which it is administered, and the fluids with which it comes in contact in the alimentary canal; and itself being slowly diffusible, may prevent the too rapid absorption of the water with which it is united. This view was first suggested by Buchheim,<sup>3</sup> but coupled by him with stimulated peristalsis. This observer failed, however, to offer the slightest experimental proof of his suggestion. If a purgative salt does act in this manner, it is evident that, if it could be administered without, or with very little, water to an animal whose alimentary canal was rendered by some means free from fluids, no purgation would follow. For this purpose Buchheim and Wagner abstained from fluids for one day, and in the course of the same day ate each a full dose of the exsiccated sulphate of soda, yet purgation followed as usual. This did not deter these observers from still clinging to the view they had formed, believing, no doubt, that they had not sufficiently freed the alimentary canal from the fluids which are usually present.

While working under Professor Schmiedeberg in his laboratory at Strassburg, I was induced by him to test Buchheim's suggestion by a stricter method, and on the lower animals. And for much valuable and kindly-given help in carrying out this the first series of experiments, I very warmly acknowledge my great indebtedness to that distinguished pharmacologist.

<sup>1</sup> *Supra*, p. 247.

<sup>2</sup> *Supra*, p. 253.

<sup>3</sup> Buchheim, *op cit.*

The salt employed was mostly the sulphate of soda, and in a few experiments the corresponding compound of magnesia. The experiments were conducted on rabbits, cats, and dogs. In ascertaining the amount of the saline eliminated in the urine and in the fæces, every precaution was taken to ensure that the whole of these excretions were obtained, and that the method of analysis was exact, and conducted with all care. With this object the animals were kept in the usual zinc boxes provided with an opening at their most dependent part for the escape and collection of the urine. In certain experiments, where great exactness was desired, the urine of the dogs was removed by catheterisation, in order to prevent its being contaminated with fæces, and to obtain, by emptying the bladder, the whole of the urine excreted by the kidneys during a stated period. In estimating the quantity of sulphuric acid in the urine, the usual method of precipitation was followed.

A given portion of well mixed urine, the remainder being retained for a second, or even third, analysis, if that were necessary, was treated with excess of hydrochloric acid, and filtered, and the filter washed until the washings gave no trace of a precipitate with barium chloride. A solution of this salt was then added in slight excess to the filtrate, and the fluid containing the precipitate of sulphate of barium was placed for some hours over the water-bath, in order to consolidate the precipitate, and prevent its passing through the filter, as it was otherwise apt, to some extent, to do. It was now filtered, and the filter containing the sulphate was repeatedly washed with hot distilled water until the washings gave no opacity with sulphuric acid, or with nitrate of silver and nitric acid. This often occupied several hours. The filtrate was tested with barium chloride to make certain that all the sulphuric acid had been precipitated. The filter with the barium sulphate was dried in a steam-bath, and burned in a platinum capsule, the ash being rendered perfectly white, if that were necessary, by the addition of a drop or two of sulphuric acid, and again burning. Cooled in a desiccator and weighed, the amount of barium sulphate was obtained after deducting the weight of the capsule and the ash of the filter paper. From this was readily calculated the amount of sulphuric acid, or of sulphate of soda, to which the weight of the barium sulphate corresponded. In almost every case the analysis was controlled by a second, and if the results of the two materially differed—more than by 0.01 gramme—a third was made.

The analysis of the fæces was attended with more trouble. Two methods were open—either to digest the fæces with acid and water, and filter and wash, and estimate the sulphates from the filtrate, or to burn the fæces and estimate the sulphates in the ash. The former

method is the more correct theoretically, as in the latter, by the burning of the fæces, a certain amount of the sulphuric acid may be decomposed in the charring, or a slight excess obtained from the oxidation of the sulphur of the albumen. The latter is, however, much more practicable than the former, as owing to the large amount of mucin present in the fæces, many days will not suffice for its perfect filtration and washing. All my analyses were accordingly made by the method of combustion, employing for this purpose a powerful oven. Previous to burning, the quantity of water in the fæces was ascertained by heating them to perfect dryness over the water-bath, and afterwards in the air-bath at a temperature of  $110^{\circ}$  C. ; and in order to prevent the volatilisation of acids a given weight of exsiccated carbonate of soda was sprinkled over the fæces before being placed on the bath. After burning, the ash was dissolved in hydrochloric acid and water, and the sulphuric acid of the filtrate estimated exactly as in the case of the urine.

I have described with minuteness these simple methods of analysis, in order to leave no room for doubt in the minds of others as to the accuracy of my work, as well as to do justice to the care with which the analyses were performed.

In all the succeeding experiments where the sulphate of soda is mentioned as being administered, the usual crystalline form of the salt ( $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ ) was that employed.

#### *Experiments on Rabbits.*

*Experiment I.*—To ascertain the dose of sulphate of soda necessary to produce purgation, when administered in the form of a dilute solution.

Rabbit, weighing 1440 grammes. Injected into the stomach through a vulcanised catheter 2 grammes of the salt, dissolved in sufficient water to form a 10 per cent. solution. On following day, no purgation ; injected 3 grammes (5 per cent. solution). At noon, next day, still no purgation ; injected other 3 grammes (5 per cent. solution). Purgation followed in two hours afterwards—copious brown watery discharge. The short interval between the catharsis and the administration of the last dose of the salt hardly permits of the latter having taken any part in the production of the former.

Five grammes, therefore, will probably purge. Even a smaller dose may act if it be still more diluted, as in

*Experiment II.*—Same rabbit as in previous experiment, after a week's interval. Administered *per os* 2 grammes ( $2\frac{1}{2}$  per cent. solution), followed in an hour afterwards by another 2 grammes, of same strength of solution. Free fluid purgation during the night.

Four grammes, in this case, well diluted, have produced active catharsis, as is confirmed by the next experiment with another rabbit.

*Experiment III.*—Rabbit, weighing 1560 grammes. Administered 2 grammes ( $2\frac{1}{2}$  per cent. solution), and in an hour afterwards the injection was repeated. Large fluid evacuation during the night.

After an interval of more than a week, this rabbit was used to ascertain if the same weight of sulphate of magnesia ( $\text{MgSO}_4 \cdot 7\text{H}_2\text{O}$ ), diluted to the same extent, would act equally powerfully.

*Experiment IV.*—Administered 2 grammes of sulphate of magnesia ( $2\frac{1}{2}$  per cent. solution), and repeated the dose in an hour. Active purging during the night.

An equal dose, therefore, of sulphate of magnesia is as purgative, when well diluted, as the same dose of sulphate of soda. It may be well to observe that there was rarely more than one fluid discharge, and that within twelve hours after purgation first occurred the stools in each instance resumed their normal firm consistence. The action of the salt is, therefore, not prolonged.

Throughout these four experiments the rabbits were fed on their usual diet of turnip and brown bread.

The effect of administering the salt in a concentrated form, and to a rabbit which had been more or less deprived of water in its food, was next studied. In Buchheim and Wagner's experiments, a single day's abstention from fluid food was apparently not sufficient to deprive the alimentary canal of its water.<sup>1</sup> Accordingly, in the following experiments, water and turnips were withheld from the rabbits for three or four days previous to the administration of a concentrated solution of the salt, the diet consisting merely of stale wheaten bread, cut from the loaf as required, without being afterwards dried, so that there was still a certain, although small, amount of water in the food. The same diet was continued throughout the experiment. By this means it was hoped to obtain the alimentary canal so free from fluid that, when the concentrated salt was administered, if Buchheim's theory were correct, purgation would not follow.

<sup>1</sup> *Supra*, p. 257.

*Experiment V.*—Same rabbit as in Experiment I. Fed as described for four days previously. Administered *per os* 5 grammes of sulphate of soda (20 per cent. solution). Next day no purgation; gave 3 grammes (20 per cent. solution). Evacuation of a small quantity of hard faeces in the evening. On the following forenoon a similar small firm stool; injected 5 grammes (20 per cent. solution). No defecation next day. One day afterwards, 80 c.c. of water were twice injected into the stomach, and the rabbit placed on its usual diet. A small amount of purging occurred in the course of the same evening and of the next day.

*Experiment VI.*—Same rabbit as in Experiment III. Fed on restricted diet during the three previous days. Administered 7 grammes of sulphate of soda (20 per cent. solution) in the forenoon; in the afternoon three or four pellets of hard faeces. Similar evacuation next day. On the third day, still no catharsis.

*Experiment VII.*—Rabbit of Experiment V. after an interval of over a fortnight. Fed as in that experiment. Administered 8 grammes of sulphate of magnesia (20 per cent. solution). Neither during that day nor the two following was there any evacuation whatever of faeces.

These experiments show most conclusively that, while 4 grammes of a purgative salt will, when freely diluted with water ( $2\frac{1}{2}$  per cent. solution), produce their usual effect, a dose twice as large, administered in a concentrated form (20 per cent. solution), and to a rabbit fed for some days previously on a water-restricted diet, is quite incapable of causing purgation.

The alimentary canal of the rabbit is in proportion to the size of the animal much longer than in the cat or dog, in which its relative length more nearly approaches that of the alimentary canal of man. Is the same result obtainable from the cat and the dog as from the rabbit? The succeeding experiments answer the question in the affirmative.

#### *Experiments on Cats.*

As with the rabbits, the purgative dose of the salt in a dilute solution and administered under ordinary conditions was first ascertained. The experiments for this purpose were numerous, as I wished to know with perfect certainty as a basis for another series of experiments what dose would, without fail, cause purgation, and yet be not excessive. Very varying results were obtained as to the length of time in which purgation took place, although the salt was given to cats of similar weight and under like conditions. This is probably due more to the degree

of restraint exercised by the animal over defecation than to a difference in the behaviour of the purgative, the cat from habits of training fearing to defecate when cooped up in the laboratory. This also applies to dogs which have been accustomed to live in the dwelling-house.

In the following experiments with cats, as well as in those with dogs, no food was given for eighteen to twenty-four hours previously, nor for a few hours after the administration of the salt, in order to ensure that the presence of food in the alimentary tract, or the process of active digestion, would not interfere with the action of the salt. Further, they were for at least a week before the experiment, fed on a regular diet, the cats on raw or boiled flesh, and the dogs on bread, and kept under observation, to render the conditions of the various experiments as equal as possible, and to make certain that the stools were of normal consistence. Water was, of course, supplied daily to the animals, unless where the conditions of the experiment required its being withheld.

*Experiment VIII.*—Grey cat, male, weighing 3.1 kilogr. Remembering that 4 grammes, well diluted, of sulphate of soda were required to purge a rabbit less than half the weight of this cat, I injected into its stomach, through a catheter, 10 grammes of the salt (5 per cent. solution). After seven hours there was a copious fluid discharge, and purgation continued for three days.

A dose of 10 grammes is apparently too large.

*Experiment IX.*—White cat, female, weighing 2.63 kilogr. Administered 3 grammes of the salt (5 per cent. solution). After ten to twelve hours a fairly abundant liquid evacuation.

*Experiment X.*—Same cat as in last experiment, after an interval of several days. Administered 3 grammes of the salt (10 per cent. solution). Partly watery, but mostly semi-fluid discharge in ten to twelve hours.

*Experiment XI.*—Black cat, male, of average size, not weighed. Gave 3 grammes of salt (10 per cent. solution). No purgation followed.

Three grammes are, therefore, insufficient to produce free purgation, although in two out of the three experiments some degree of action followed. I, accordingly, judged that 5 grammes would, in all cases, result in decided catharsis, a supposition confirmed by the next experiments.

*Experiment XII.*—Grey cat, female, weighing 2·74 kilogr. Administered 5 grammes of sulphate of soda (5 per cent. solution). Fluid evacuation, tolerably profuse, in about five hours.

*Experiment XIII.*—Black cat, male, weighing 3·45 kilogr. Administered 5 grammes of salt (10 per cent. solution). Purgation after twelve hours.

*Experiment XIV.*—Tortoise-shell cat, male, weighing 3·00 kilogr. Same dose as in previous experiment, but 20 per cent. solution. Purgative effect followed in fifteen or sixteen hours.

Many other experiments were made with the same dose of 5 grammes of sulphate of soda; and if the cat were fed on its usual diet, however concentrated might be the solution of the salt, purgation invariably resulted. But it was observed, as the three last experiments show, that, as a rule, the more dilute the solution, the more quickly was the action produced. Further, it was found that the dose varied extremely little with the size or weight of the animal, as large a dose being required to purge a cat of 2 kilogr. as one of 4 kilogr. This I have observed most strikingly in dogs, where the difference in size is much more widely variable; a small terrier requiring nearly as much as a large mastiff perhaps four or five times the weight of the smaller dog.

As to the effect of restriction of the water supply in the diet for two or three days previously, combined with the administration of a concentrated solution of the salt, the succeeding experiments prove that it is much the same with cats as with rabbits.

*Experiment XV.*—Black cat, same as in Experiment XIII. Fed for the last three days on bread only, very little of which it ate. Administered 5 grammes of sulphate of soda (20 per cent. solution). Faeces, next day, somewhat softer than usual, but still retaining their ordinary cylindrical form, and by no means fluid or even semi-fluid. Never any purgation.

*Experiment XVI.*—Grey cat, same as in Experiment XII. Fed only on bread for four days previously. Gave 5 grammes of the salt (20 per cent. solution). No evacuation during the next forty-eight hours; perfectly firm when it did appear.

*Experiment XVII.*—Tortoise-shell cat, same as in Experiment XIV. Water excluded from diet for three days previously. Salt administered in equal dose and concentration to that in last two experiments, and with an exactly similar result.

*Experiments on Dogs.*

I finally repeated these experiments on dogs in order to obtain all the confirmation possible of the influence exercised by the presence of water in the digestive tract on the action of a saline cathartic. These animals were particularly suitable, as they enabled me at the same time to make some precise analyses of the quantity of the salt recoverable in the urine and the fæces. Some analyses I had made of the urine of the rabbits, but as these were not quite so accurate owing to the difficulty of emptying the bladder at the end of every twenty-four hours or other stated period, and, as in the main, their results agree with those obtained from dogs, I have not thought it worth while to give them. The same objections were open to analyses of the cats' urine, as their urethra is too narrow to admit of the easy introduction of a catheter. On the other hand, a suitable catheter could with the greatest facility be introduced through the wider urethra of the dog, especially of the male, the female not being so convenient for this purpose.

The first experiments were made with the object of ascertaining what dose of the salt was able, under ordinary conditions of dilution and of supply of water in the diet, to produce catharsis; also, how much of the salt was eliminated in the urine, and how much in the fæces. Two dogs only were used, and both of them males. Neither of them, unfortunately, was weighed; the one was rather larger than a sheep dog, and the other was a large-sized terrier. Both tended to be a little constipated, probably from long confinement in the laboratory previous to their being employed for my experiments. Their usual diet was raw flesh and bread, and water *ad libitum*; but for some days previous to an experiment they were fed on bread and water only, in order to obtain fæces comparable in composition with those evacuated whilst they were confined to bread solely as was necessitated in the experiments requiring absence of water from the food, flesh being then inadmissible on account of its containing too high a percentage of water. As previously mentioned, no food was given on the morning of the experiment; they were, fed, however, in the course of the day.

Before commencing the administration of the salt, I esti-



mated in each of the dogs the normal daily quantity of sulphates in the urine and in the fæces.

*Experiment XVIII.*—The small dog. Fed for the previous week, and during the experiment, on bread and water. At 6.30 P.M., emptied the bladder by means of the catheter, and placed the dog in a zinc cage, so constructed that the urine evacuated was collected in a receiver beneath. Next day, at 6.20 P.M., again catheterised, and the urine obtained added to that in the receiver. The whole measured 527 c.c., and yielded by the careful method of analysis already described 0.281 gramme of sulphuric acid ( $\text{H}_2\text{SO}_4$ ), corresponding to 1.0255 grammes of crystalline sulphate of soda ( $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ ).

On the third day, catheterised at the same hour as before, the whole urine measured 509 c.c., and yielded 0.276 gramme of  $\text{H}_2\text{SO}_4$ , equivalent to 0.906 gramme of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ .

As the fæces were evacuated somewhat irregularly, they were collected for four complete days, commencing four days after the beginning of the bread and water diet. The whole weighed 139.40 grammes. After drying over water-bath, and in air-bath at  $110^\circ \text{C}$ ., the weight was reduced to 52.165 grammes, corresponding to 62.58 per cent. of water. After complete combustion in a furnace, the ash amounted to 4.42 grammes, and contained 0.3081 gramme of  $\text{H}_2\text{SO}_4$ , equal to 1.0124 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ .

The daily average, therefore, of the sulphuric acid in the fæces of the small dog, reckoned in the form of the sulphate of soda, was 0.2531 gramme, and in the urine  $\frac{1.0255 + 0.906}{2}$ , or 0.9657 gramme.

*Experiment XIX.*—The large dog. Same arrangement of dietary as in foregoing experiment. Emptied bladder at 5.10 P.M. At same hour on following day, bladder again emptied, and urine mixed with that in the receiver; the whole measured 415 c.c., and yielded 0.5481 gramme of  $\text{H}_2\text{SO}_4$ , equivalent to 1.801 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ .

The fæces which were evacuated more regularly than by the small dog were collected for three days; and as I wished to know with accuracy the percentage of water in the freshly-passed stool, each evacuation was weighed as obtained, in order to avoid error from loss by evaporation while standing exposed to the air. The whole fæces for the three days were of the usual firm consistence, and weighed 182.67 grammes, and after drying, 69.78 grammes, and therefore contained 60.7 per cent. of water. The ash amounted to 5.76 grammes, and yielded 0.3194 gramme of  $\text{H}_2\text{SO}_4$ , equivalent to 1.0497 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ .

The daily average of sulphuric acid, estimated as crystalline sulphate of soda, was, accordingly, in the fæces of the large dog, 0.3499 grammes, and in the urine, 1.801 grammes.

These preliminary estimations having been made, I proceeded to ascertain the purgative dose of the sulphate of soda for each of the dogs under ordinary conditions, and to estimate the quantity of the salt recoverable from the urine and the fæces.

*Experiment XX.*—Small dog. Injected *per os* into its stomach 10 grammes of sulphate of soda (5 per cent. solution). This dose exercised no purgative effect; the fæces evacuated during the three subsequent days were of ordinary consistence.

Ten grammes of the salt were insufficient to produce purgation in the small dog.

*Experiment XXI.*—Same dog. Administered 15 grammes (5 per cent. solution). In fourteen and a half hours there was a copious discharge of a dark-brown, watery, gruel-like fluid. No purgation, and even no fæces, during the next two days.

Fifteen grammes were apparently a good purgative dose; and this was confirmed by the next experiment, where the salt was given in a more concentrated form.

*Experiment XXII.*—Same dog. Administered 15 grammes (20 per cent. solution). Purgation in eighteen and a half hours, but stool much less watery and less abundant than in Experiment XXI. As, in that experiment, no fæces were passed during the next two days.

The effect of the concentration of the salt in lengthening the time in which purgation took place, and in lessening the degree of purgation, was as remarkable in the dog as in the cat and rabbit.

That fifteen grammes sufficiently diluted were capable at all times of causing a profuse fluid discharge from this dog was proved by several other experiments, one of which I have selected, accompanied by analyses of the urine and fæces.

*Experiment XXIII.*—Small dog. Fed, during the experiment, on bread and water, and for three days previously. 9.15 A.M.—Bladder emptied, and 15 grammes of the sulphate of soda (4 per cent. solution) injected *per os* into stomach. The urine was removed by the catheter from the bladder every three hours, in order to prevent its mingling with the fluid fæces which might at any time be discharged. Purgation at 9 P.M., abundant and fluid. Next morning, at 9.15, the bladder was catheterised. No urine was passed during the night, and purgation had not again occurred. The whole urine of the twenty-four hours amounted to 765 c.c., and contained sulphuric acid equivalent to 5.9945 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , which, after the deduction of the sulphuric acid normally present, shows an excess of 5.028 grammes of the sulphate.

The purgative stool passed the first evening weighed 366·8 grammes, and lost by drying 334·6 grammes, and, therefore, contained 91·05 per cent. of water. By complete combustion the weight was reduced to 5·20 grammes. This yielded sulphuric acid corresponding to 8·485 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , or an excess of 8·2319 grammes.

Next day, at 9.15 A.M., the urine was again withdrawn from the bladder, and added to what had been passed in the usual way during the previous twenty-four hours. It measured 1090 c.c., and contained sulphuric acid equivalent to 1·4758 grammes of  $\text{Na}_2\text{SO}_4 \cdot 18\text{H}_2\text{O}$ , or an excess of 0·5101 gramme.

The urine of the third twenty-four hours was obtained in the same manner, and amounted to 612 c.c. It yielded 0·9131 gramme of the sulphate, and, therefore, no excess. The whole of the salt had now been eliminated.

Since the purgative evacuation, no discharge of fæces occurred until between sixty and seventy hours after the administration of the salt. The discharge was soft but solid, and weighed 66·175 grammes, and after drying, 18·9 grammes, thus containing 71·43 per cent. of water. The ash weighed 1·410 grammes, and yielded sulphuric acid equal to 1·046 grammes of sodic sulphate, or an excess of 0·7929, if we regard this small quantity of fæces as not equivalent to more than a single day's evacuation, although it was actually all that was obtained for two days. If we consider it as representing the fæces of two days, then the total excess during that time is reduced to 0·5398 gramme.

A further evacuation of solid fæces occurred eighty-three hours after the administration of the purge. It weighed 28·25 grammes, and was reduced by drying to 10·875 grammes, showing that 61·51 per cent. of water was present. The sulphuric acid was not estimated.

Experiments of the same nature were made on the large dog, and with much the same results.

*Experiment XXIV.*—Large dog. Fed on bread and water for some days before, and during, the experiment. Administered, at 5.15 P.M., 16 grammes of sulphate of soda (about  $7\frac{1}{2}$  per cent. solution). At 11 A.M., on the following day, there was an evacuation mainly of firm fæces, but mixed with a small quantity of watery fluid. The whole weighed 77 grammes, and after drying, 30·240 grammes, and contained, therefore, 60·74 per cent. of water. The ash weighed 3·59 grammes, and yielded of  $\text{H}_2\text{SO}_4$ , calculated as  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , 1·202 grammes, or the small excess of 0·852 grammes. As part of the urine was lost it was not examined.

A dose of 16 grammes was evidently too small for so large a dog, and, accordingly, 18 grammes were administered in the next experiment.

*Experiment XXV.*—Same dog. Fed as in preceding experiment. At 12 noon, emptied the bladder, and injected into the stomach 18 grammes of the salt (6 per cent. solution). The dog licked up a large quantity of water shortly afterwards.

In the course of next forenoon there was evacuated a large quantity of brown perfectly fluid *faeces*. At noon the bladder was catheterised. The whole twenty-four hours' urine measured 925 c.c., and contained  $\text{H}_2\text{SO}_4$  equivalent to 9.050 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , or an excess of 7.249 grammes. The *faeces* weighed 143.36 grammes, and after drying, 23.954 grammes, and thus contained 83.30 per cent. of water. Of ash there were 5.744 grammes, which yielded  $\text{H}_2\text{SO}_4$  corresponding to 6.818 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , or an excess of 6.468 grammes.

On the following day, at 12.10 P.M., the bladder was catheterised and the urine collected as previously. It measured 657 c.c., and gave  $\text{H}_2\text{SO}_4$  equal to 3.413 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , or an excess of 1.612 grammes.

On the same day, at 12.20 P.M., the dog evacuated 107.5 grammes of nearly solid *faeces*, which, when dried, weighed 26.01 grammes, equal to 75.81 per cent. of water. The ash weighed 2.90 grammes, and contained  $\text{H}_2\text{SO}_4$  equivalent to 1.885 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , or an excess of 1.535 grammes.

Next day, at 12.15 P.M., the urine was collected in the usual manner. It measured 1060 c.c., and contained  $\text{H}_2\text{SO}_4$ , estimated as  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , 1.7235 grammes, or no excess; on the contrary, the sulphuric acid was slightly under the ascertained normal quantity.

The *faeces*, evacuated in the course of the forenoon of this day, were, although softer than usual, not so much so as to be pulpy. Their weight was 123.7 grammes, and, when dried, 24.17 grammes; consequently, 80.4 per cent. of water was present. Weight after combustion was 1.95 grammes; and the ash yielded  $\text{H}_2\text{SO}_4$  equivalent to 0.4968 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , or the trifling excess of 0.0469 grammes.

Eighteen grammes of sulphate of soda proved, therefore, amply sufficient to purge the large dog. Of course, in all these experiments, a sufficient interval of time, never less than one week, was allowed between them, so that the effects of the one experiment might not interfere with the condition of the animal in the succeeding experiment.

The next experiments are designed to show, what I have already ascertained for rabbits and cats, that, if dogs be fed on a diet tolerably free from water during, and for a day or two before, the administration of the purgative salt, and if, at the same time, the salt be given in a concentrated state, purgation will not ensue.

*Experiment XXVI.*—Small dog. Fed on bread alone for the two previous days and throughout the experiment; no water. At 5.40 P.M., administered 15 grammes of dry crystalline sulphate of soda, made into large pills or boluses with a little bread and a very few drops of a solution of gum-arabic. The dog's mouth was opened and

the bolus thrust well back into the pharynx, when it was easily swallowed.

As this was one of my earliest experiments I had not yet commenced to withdraw the urine from the bladder: and as micturition did not occur during the whole of the first night and the following day, there was no urine to examine. During the same time there were also no fæces.

On the forenoon of the second day after the administration of the salt, the urine found in the collecting vessel beneath the zinc cage measured 239 c.c., and contained a quantity of  $\text{H}_2\text{SO}_4$  equivalent to 13.640 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , or the large excess over the two days of 11.708 grammes.

During the same forenoon, 43.58 grammes of solid dry fæces were passed, which, after dessication, weighed 23.06 grammes, equal to 47.09 per cent. of water. The ash weighed 4.645 grammes, and yielded  $\text{H}_2\text{SO}_4$  equivalent to 0.3540 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , or the small excess of 0.1009 grammes.

On the forenoon of the third day, the urine was again collected, and measured 80 c.c. It contained  $\text{H}_2\text{SO}_4$  corresponding to 1.480 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , or an excess of 0.5143 grammes.

The fæces passed during the same forenoon weighed 81.2 grammes; although consisting almost wholly of ordinary firm material, a small portion was somewhat softened, but not semi-fluid. The residue after drying amounted to 22.78 grammes, and thus contained 71.95 per cent. of water. The ash weighed 3.941 grammes, and contained 1.792 grammes of  $\text{H}_2\text{SO}_4$ , reckoned as  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , or an excess of 1.539 grammes.

Water was now freely added to the dog's diet. Next day, the urine measured 480 c.c., and contained  $\text{H}_2\text{SO}_4$  corresponding to 3.158 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , or an excess of 2.192 grammes. There was a moderate quantity of fæces of ordinary consistence, which was not analysed.

*Experiment XXVII.*—Large dog. Diet of bread during and for one day previous to the experiment; no water. At 5 P.M., emptied the bladder, and administered 18 grammes of crystalline sulphate of soda in the form of boluses as in foregoing experiment.

Next day, at 5 P.M., no fæces; bladder emptied. Whole urine measured 375 c.c., and contained 15.9125 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , estimated from the  $\text{H}_2\text{SO}_4$ , or an excess of 14.111 grammes.

On the following day, at noon, there was an evacuation of fæces of ordinary firm consistence, which weighed 44.6 grammes, and, after dessication, 20.75 grammes, equivalent to 53.48 per cent. of water. The ash weighed 4.236 grammes, and yielded  $\text{H}_2\text{SO}_4$  corresponding to 0.3033 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , or no excess of the salt.

At 5 P.M., on the same day, the bladder was as usual emptied. The urine measured altogether 148 c.c., and, estimated from the  $\text{H}_2\text{SO}_4$  present, contained 1.5037 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , or no excess.

During the next, or third, day after the administration of the purge there was no faecal discharge.

*Experiment XXVIII.*—Large dog. Simply a repetition of the preceding experiment, the dose of the salt and the conditions of the diet being exactly the same. The urine during the first twenty-four hours amounted to 282 c.c.; and no fæces were evacuated until twenty-seven hours after the administration of the salt. The fæces were of the usual firm consistence.

These experiments with dogs confirm those made with rabbits and cats, and lead to the important result, the demonstration of which has been the principal object of this series of experiments, that a purgative salt, as sulphate of soda, will not produce catharsis if it be given in a state of concentration, and if means be taken to ensure for even a single day previous to the administration of the salt that little or no water is taken with the food. These precautions are intended, as previously explained, to free the alimentary canal as far as possible from fluids.

This result seems to warrant the deduction, that a saline cathartic acts, neither by exciting peristalsis, nor by increasing the intestinal secretions, but merely by uniting with the water in which it is dissolved, and with the fluids which it meets in the alimentary canal, preventing the absorption and preserving the fluidity of the intestinal contents, until the ordinary peristaltic movements have carried them to the rectum. Whether this theory be correct, and with what reservations these experiments must be accepted in its support, I shall fully discuss in the course of the numerous series of experiments which have yet to follow. Meanwhile, it may be affirmed that, by an extension and more strict application of the method of Buchheim, I have succeeded where he failed in establishing proof of the theory he advanced. Two qualities are assumed for purgative salts; one of these, they have already been shown to possess by Graham and others, viz., a low diffusive power; the other quality, a strong affinity for water, is, although generally admitted, extremely doubtful, as I shall afterwards take occasion to point out.

Another result of these experiments, and one of some therapeutical value, is, that the rapidity of action of the salt, and the extent of its action, bear a certain proportion to the degree of its dilution. This is borne out by the experiments in all of the three kinds of animals used, and is perfectly consonant with the theory presently under discussion. The fact is not novel,

although in practice often forgotten, and was perfectly well known even to Hippocrates, and apparently applies to vegetable as well as to saline cathartics. We find it expressed in an aphorism of this venerable physician—"Corpora ubi quis purgare voluerit, facile fluentia reddere oportet."<sup>1</sup>

I shall now pass to a consideration of the objections which can be urged against the method I have employed in this series of experiments. It might be said, and with much fairness, that, by a restriction of water in the diet of my animals, I not only freed the alimentary canal from its fluids, but also largely diminished the quantity of water in the blood; so that a concentration of the blood was produced, which did not permit of the usual intestinal secretions being poured out when the glands were stimulated by the presence of the salt. If this be true, it will follow, that, if we feed an animal on a water-restricted diet for some time in order to diminish the fluids within the alimentary canal as far as possible, and, at the time of administration of the salt, inject into the blood a large quantity of water, the condition of concentration being thus overcome, purgation should occur. I made such an experiment with a rabbit, and found that purgation did not ensue, as the following protocol evidences:—

*Experiment XXIX.*—Rabbit, weighing 1605 grammes, which was freely purged some days before with 4 grammes of sulphate of soda dissolved in a large quantity of water. No water was given for two days previous to this experiment, and the diet consisted of bread alone. At 4.10 P.M. administered *per os* the excessive dose of 8 grammes of crystalline sulphate of soda (20 per cent. solution). 4.25 P.M., commenced injecting into the right jugular vein a warm (35°–40° C.)  $\frac{3}{4}$  per cent. solution of chloride of sodium. The solution was allowed to run slowly and continuously into the vein from a burette connected with it, and provided at its lower end with a stopcock for the regulation of the flow. The injection was completed at 7.40 P.M., when 200 c.c. of the solution had been introduced. I feared that, if I had injected the fluid more quickly, it might have been rapidly eliminated by the kidneys, without the salt in the digestive tube having had time to attract sufficient of it to cause purgation. The rabbit micturated twice during the operation, and again immediately after being returned to its box. All the urine passed during the operation was carefully collected with a clean sponge. On being released from the holder, the animal jumped away quite briskly, apparently uninjured by the injection of the large quantity of fluid—nearly twice the total quantity of its blood.

Next day, at 10 A.M., it was found that during the night some faeces

<sup>1</sup> Hippocrates, *Aph.* 9, sect. ii.

had been evacuated, for the most part firm, but mixed with a small quantity of semi-fluid material. There was no purgation, such as might have been expected to follow the administration of a dilute solution of so large a dose of the salt. The urine up to this hour, including that collected yesterday evening, measured 190 c.c., and yielded  $\text{H}_2\text{SO}_4$ , corresponding to 4.585 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , or an excess not exceeding 4 grammes.

On the following day, no further evacuation of fæces. Urine measured 33 c.c., and was thickish and somewhat gelatinous. The diet was still water-free. The urine gave  $\text{H}_2\text{SO}_4$ , equivalent to 1.637 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , or an excess, probably, of about 1.25 grammes.

Although this rabbit was kept under observation for several days more, the fæces never became fluid. The urine, very strangely, became more and more gelatinous; and, on the fifth day from the beginning of the experiment, was so much so as to form a soft gelatine-like jelly, perfectly transparent and of a pale yellow colour. On heating it became fluid, and showed no trace of coagulum. It gave a dense whitish precipitate with tannic acid, which partly dissolved in ammonia, and the filtrate from the solution of the precipitate in ammonia yielded an abundant precipitate with hydrochloric acid. By treating the latter precipitate with oxide of lead, the tannic acid was removed, and there remained after filtration and evaporation of the filtrate a small quantity of a gelatinous material. Whether it was real gelatin or not, the quantity was too small to permit of this being determined by an analysis of its chemical composition. It was probably gelatin. Although water was freely added to the diet, the gelatinous condition of the urine persisted.

The noteworthy result of this experiment is, that the injection of the water into the circulation seems hardly to affect the inaction of the concentrated solution of the salt in the alimentary canal—another proof that the salt is not capable of withdrawing water from the blood through the medium of the intestinal glands, or otherwise, but simply combines with the water it finds already present in the canal. There was certainly a small quantity of semi-fluid fæces, but the amount of purgation is not at all equal to the dose of the purgative administered.

The appearance of gelatine, or a gelatine-like substance, in the urine of this rabbit, although of no importance in connection with the object of the present investigation, is a very unusual and extremely interesting phenomenon, and, so far as I know, a phenomenon hitherto unobserved. It was probably produced by the conditions of the experiment, but how, and by which, or by all, of these conditions I cannot attempt to say.



Vulpian<sup>1</sup> has asserted that a purgative salt acts chiefly by exciting an inflammatory irritation of the alimentary mucous membrane, and consequently creates an exudation such as we observe in a mucous catarrh. Since the irritant action of croton oil is visible even on the skin, we can hardly doubt that the oil also irritates the mucous membrane when in contact with it. But, if the salt and the oil both act in the same manner, an ordinary purgative dose of each should be similarly modified by similar circumstances. If the salt fails to excite secretion, or, at least, produce purgation in an animal deprived of water, equally, should croton oil be without effect, when administered to an animal in the same condition. Is this so?

*Experiment XXX.*—Small dog. Deprived of water for the previous two days. At 5.10 P.M. administered 5 drops of croton oil in the form of a pill, a dose ascertained under ordinary conditions to produce moderate purgation. A much larger dose is required for the dog than for man. By nine on the following morning, free purgation had occurred.

Croton oil, therefore, can purge where sulphate of soda fails; and the conclusion is evident, that, if the oil acts by causing an inflammatory exudation from the alimentary mucous membrane, the salt does not act in the same manner, and as Vulpian suggests. We are, further, tempted to infer from this experiment that as croton oil is still capable of withdrawing fluid from the blood when the animal has been deprived of its water for a day or two previously, so sulphate of soda, acting under the same condition, should not be prevented from extracting water from the blood, did it ordinarily exercise such an action, whatever be the exact nature of that action. If this inference be legitimate, it lends support to the theory at present being tested. But, it is possible, notwithstanding the result of Experiment XXIX., that, if the salt excite a true secretion and not an inflammatory exudation, a concentrated condition of the blood, arising from a deficiency of water in the diet, may materially diminish the former, while it is unable to any great extent to modify the quantity of the latter.

The experiments hitherto detailed are sufficient to give a *locus standi* to the theory which they were designed to support

<sup>1</sup> Vulpian, *op. cit.*

—that a salt purges in virtue of its combining with the water in the alimentary canal, thus preventing the rapid absorption of the water on account of its own low diffusive power. This was my object when I commenced this investigation. And, indeed, I had hoped to find in this theory a complete explanation of the action of saline purgatives. So far I appear to have succeeded. But it may be well at this stage of the inquiry to frankly admit that the experiments of the various succeeding series have compelled me, by no means unwillingly—for I but sought the truth—to abandon the present view; for what reasons I shall afterwards state.

This admission does not necessarily impair the value of the preceding experiments. They are of themselves of much pharmacological and physiological interest; and the several analyses of the excretions offer some facts of both practical and scientific importance. These we will now consider. For their more convenient comparison, I have arranged them in a tabular form. I need hardly repeat that they were made with the greatest possible care and exactitude, each analysis in almost every instance having been performed twice. We shall first examine the effect of the salt on the urine, and afterwards its effect on the *faeces*.

The experiments tabulated exhibit a material increase in the quantity of the urine after the administration of the salt in a diluted form, probably in part due to the water given with the salt. As from other experiments, which will afterwards be given, I have observed that the increase is usually best marked on the day following the ingestion of the salt, I may be permitted here to point to Experiment XXIII. as showing this increase very clearly. In the corresponding experiment with the large dog (Experiment XXV.), this rise does not occur on the second day, but on the third, while there is a very large excretion of urine during the first day. This anomaly is probably the result of the unusually large quantity of water which was taken with the salt.

Where the salt has been administered without water to a dog fed on a water-restricted diet, the urine, as might have been expected, is largely diminished in quantity, the effect, in all likelihood, of the mere modification of the diet.

## ANALYSIS OF THE URINE AND FEACES OF THE SMALL DOG.

No. of Experiment	Diet.	Dose of $\text{Na}_2\text{SO}_4$ , 10Aq.	Strength of solution.	Time in which first evacuation occurred.	Day after Administration.	URINE.			FECES.					
						Quantity.	Total Weight of $\text{H}_2\text{SO}_4$ as $\text{Na}_2\text{SO}_4$ , 10Aq.	Excess of $\text{Na}_2\text{SO}_4$ + 10Aq.	Weight.	Weight after drying at $110^\circ \text{C}$ .	Per-centage of Water.	Weight of Ash.	Total Weight of $\text{H}_2\text{SO}_4$ as $\text{Na}_2\text{SO}_4$ + 10Aq.	Excess of $\text{Na}_2\text{SO}_4$ + 10Aq.
XVIII.	{ Bread and water. }	grma.	normal	...	I.	c.c.	1.025	...	grma.	...	...	...	...	...
	"	...	...	...	II.	509	0.906	...	Average of four days.	...	...	...	...	...
	{ Bread and water. }	15	2 %	12 hours.	I.	765	5.9945	5.0280	366.8	32.2	91.05	5.2	8.4850	8.2319
XXIII.	"	...	...	...	II.	1090	1.4758	0.5101	} 66.175	18.9	71.43	1.41 or for two days.	1.046	0.7929
	"	...	...	III.	612	0.9131	...	...						
	"	...	...	...	IV.	...	...	...	28.25	10.875	61.51	...	...	...
XXVI.	{ Bread, no water. }	15	dry.	...	I.	} 239	13.640	for two days.	43.58	23.06	47.09	4.645	0.3540	0.1009
	"	...	45 hours. (†)	II.	...		11.708	...	...	...	...	...	...	...
	"	"	...	...	III.	80	1.480	0.5143	81.20	22.78	71.95	3.941	1.792	1.539
	Water added.	...	...	...	IV.	480	3.158	2.192	...	...	...	...	...	...

## ANALYSIS OF THE URINE AND FÆCES OF THE LARGE DOG.

No. of Experiment.	Diet.	Dose of $\text{Na}_2\text{SO}_4 \cdot 10\text{aq.}$	Strength of Solution.	Time in which first evacuation occurred.	Day after Administration.	Urine.			Fæces.					
						Quantity.	Total Weight of $\text{H}_2\text{SO}_4$ as $\text{Na}_2\text{SO}_4 \cdot 10\text{aq.}$	Excess of $\text{Na}_2\text{SO}_4$ + $10\text{Aq.}$	Weight.	Weight after drying at $110^\circ \text{C.}$	Per-centage of Water.	Weight of Ash.	Total Weight of $\text{H}_2\text{SO}_4$ as $\text{Na}_2\text{SO}_4 \cdot 10\text{aq.}$	Excess of $\text{Na}_2\text{SO}_4$ + $10\text{Aq.}$
XIX.	{ Bread and water. }	...	normal	...	I.	c.c. 415	1·801	...	grms. average of three days. 60·89	23·26	60·70	1·92	0·3499	
XXV.	{ Bread and water. }	18	3% (?)	18-20 hrs.	I.	925	9·050	7·249	143·36	23·954	83·80	5·744	6·818	6·468
XXVII.	{ " "	...	...	...	II.	657	3·413	1·612	107·5	26·01	75·81	2·90	1·385	1·535
	{ " "	...	...	...	III.	1060	1·7235	...	123·7	24·17	80·4	1·95	0·4968	0·0469
	{ Bread, no water. }	18	dry	...	I.	375	15·9125	14·111	{ 44·6 }		20·75	4·236	0·3033	
	{ " "	...	...	48 hrs.	II.	148	1·5087	...	{ 44·6 }		20·75	4·236	0·3033	
SMALL DOG.														
XXXI.	{ Bread and water. }	16	4½%	hrs. mins. 10 35	h. m. 10 35	253	5·478	4·503						
XXXII.	{ Bread, no water. }	16	dry	...	h. m. 10 35	138	10·030	9·065						

The salt recovered from the urine, as estimated from the excess of sulphuric acid present, is of more interest. In no case was the whole of the salt administered again obtained from the urine and the fæces. More was always recovered after a dilute than after a concentrated purge. The blood in the latter case seemed to retain the salt for a much longer time than in the former, as was proved in Experiment XXVI., by a considerable excess of the sulphate appearing in the urine after the administration of water, without that the quantity of the urine itself was increased beyond the normal. The time usually required for the elimination of the salt by the kidneys extended over more than twenty-four hours, and the small quantity obtained during the next twenty-four hours renders it highly probable that, under ordinary circumstances, the elimination is completed in from thirty to thirty-six hours from the time of administration.

As regards the amount of the salt recovered from the urine, compared with that obtained from the fæces, from one-half (Experiment XXV.) to nearly one-third (Experiment XXIII.) of the total salt excreted appeared in the urine, when the salt was administered well diluted; and the more pronounced the purgation, the less was the quantity of the salt. On the other hand, when the salt was given in a concentrated form, practically the whole of it passed into the urine. Even did absorption of the salt proceed with equal pace in both classes of experiments, this great difference might be accounted for in this way, that in the experiment with the diluted salt, *e.g.*, Experiment XXIII., a great part of the salt is removed from the alimentary canal with the stool which was evacuated twelve hours after the administration of the purgative, whereas, had purgation not occurred until the end of twenty-four hours, the most of this salt might have been absorbed from the intestines. In other words, the salt does not remain long enough in the alimentary canal to permit of its complete absorption by the blood. In Experiments XXVI. and XXVII., where the salt was given without water, no fæces were passed until the expiry of two complete days, and thus abundance of time was allowed for the disappearance of salt from the canal. The difference can certainly be accounted for in this manner; but the following two experiments will distinctly prove that, apart from the variation in the time allowed for the absorption

of the salt, the more concentrated salt is more rapidly absorbed than the more dilute. This might almost have been taken for granted, were it not that both Aubert<sup>1</sup> and Buchheim<sup>2</sup> expressly state that the extent of dilution of the salt has little or no effect on the rate of its absorption, even when some attempt was made to restrict the water in the diet (Buchheim).

*Experiment XXXI.*—Small dog. Diet, bread and water. At 9.50 A.M., emptied the bladder, and injected into the stomach 16 grammes of sulphate of soda, dissolved in 350 c.c. of water (about  $4\frac{1}{2}$  per cent. solution). The urine was taken from the bladder at intervals of three hours during the remainder of the day. At 8.25 P.M. free liquid purgation occurred. Bladder was immediately catheterised. The whole urine, from 9.50 A.M. until now, measured 253 c.c., and contained  $\text{H}_2\text{SO}_4$  equivalent to 5.478 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , or an excess of 4.503 grammes.

Purgation occurred in ten hours thirty-five minutes. In the next experiment, made several days afterwards, exactly the same dose of the salt was administered, but without water, and after the dog had been fed on a water-restricted diet. At the end of the same time, ten hours thirty-five minutes, the urine was collected and analysed.

*Experiment XXXII.*—Same dog. Fed for two days before on bread only—no water. At 9.50 A.M., bladder emptied; administered 16 grammes of the salt, in the form of large pills, made with a few crumbs of bread. At 8.25 P.M. the bladder was again catheterised. This urine, with that obtained since morning, measured 138.5 c.c., and yielded  $\text{H}_2\text{SO}_4$ , corresponding to 10.030 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , or an excess of 9.065 grammes. Purgation never occurred.

The salt recovered from the urine in the second experiment was, therefore, twice as much as in the first.

These two experiments decisively prove what the others more or less indicate, that the rapidity of the absorption of the salt is influenced by the degree of concentration of its administration, concurrently with a diminution of the fluidity of the contents of the alimentary canal, and probably also of the fluidity of the blood. The last factor, I am inclined to think from later experiments, is, perhaps, the most important.

We shall now turn to the analysis of the fæces, and it will be found that they present us with some facts of considerable value. The analysis from the large dog are more satisfactory than those

<sup>1</sup> Aubert, *op. cit.*

<sup>2</sup> Buchheim, *op. cit.*

from the small dog, as it was of a less constipated habit of body, and, under ordinary conditions, defæcated once a day. The small dog, on the contrary, rarely had stools oftener than once in two or three days, which accounts for the irregularity in the quantity of its fæces. Fortunately, it hardly affects the relative proportions of the constituents of the stools, for the normal excrements of both dogs are remarkably alike in composition, as is more clearly brought out by comparing the percentages of the constituents:—

	Small dog. <i>Average of four days.</i>	Large dog. <i>Average of three days.</i>
Water, . . .	62·58 per cent.	60·70 per cent.
Solids, . . .	37·42    "	39·30    "
"   Organic,	34·27    "	36·15    "
"   Inorganic,	3·15    "	3·15    "
H <sub>2</sub> SO <sub>4</sub> , . . .	0·220    "	0·174    "

These analyses were made, as has been previously mentioned, while the dogs were fed on brown bread and water, the diet employed, with or without the water, in all the experiments. It is satisfactory to observe this uniformity of composition, as greater weight can be attached to the variations produced in the faecal constituents by the action of the purgative. Among these variations, the most notable is naturally that of the water. Beneath 80 per cent. of water the fæces are of plastic consistence; with each additional per cent. of water they become rapidly more liquid. The amount of water, it will be observed, corresponds tolerably with the quantity of the purgative salt in the stools—the more salt, the more water. The salt, in its turn, is less, the longer the interval between its administration and its purgative effect, as, *cæteris paribus*, more of it will have been absorbed from the alimentary tract; so that, within certain limits, the shorter that interval is, the more marked is the purgative effect. This is borne out by a comparison of Experiments XXIII and XXV., and by other experiments not given. No one will deny its truth when applied to intervals of one or two days; but I have every reason to believe it is equally true for periods of almost as few hours. This is practically stating that the maximum of fluid within the canal occurs shortly after the

ANALYSIS OF THE FÆCES, SHOWING INCREASE OF THE FÆCAL ASH AFTER THE ADMINISTRATION OF A SALINE PURGATIVE (SULPHATE OF SODA).

Number of Experiment.	Nature of Experiment.	Day after Administration of Salt.	I. <sup>1</sup>			II.			III.			IV.	V.		VI.	VII.	VIII.		IX.
			Solids of Faeces.			Ash of Faeces.							Real Weight of Organic Constituents.	Percentage in Solids (III.) of					
			Total Weight.	Weight of Excess of $\text{Na}_2\text{SO}_4 \cdot 10 \text{ Aq.}$	Real Weight.	Total Weight.	Weight of Excess of $\text{Na}_2\text{SO}_4$ (water-free).	Real Weight.	Real Weight of Organic Constituents.	Inorganic Constituents.	Organic Constituents.								
XIX.	Normal.	{ Average of three days. I. II. III.	23.26	...	23.26	1.92	...	1.92	...	1.92	21.34	8.254	91.746.						
XXV.	{ Salt with water. ... ... Salt without water.		23.954	6.468	17.486	5.744	2.8529	2.891	14.595	16.538	88.467								
"			26.01 24.17	1.535 0.046	24.475 24.124	2.900 1.950	0.6769 0.0206	2.223 1.929	22.252 22.195	9.083 7.997	90.917 92.003								
XXVII.	{ Salt without water. Salt and water, but only small amount of purgation.	{ Average of four days. I. II. and III. I. and II. III.	20.75	...	20.75	4.236	...	4.236	...	4.236	16.514	20.414	79.586						
XXIV.			30.240	0.852	29.388	3.590	0.3757	3.214	26.174	10.987	89.068								
XVIII.	Normal.		13.41	...	13.41	1.10	...	1.10	12.31	8.202	91.798								
XXIII.	{ Salt with water. ... Salt without water.	{ I. II. and III. I. and II. III.	32.20	8.2319	23.968	5.20	8.627	1.573	22.395	6.562	98.438								
"			18.90	0.5398	18.360	1.41	0.238	1.172	17.188	6.392	93.618								
XXVI.			23.06	0.101	22.959	4.645	0.044	4.601	18.358	20.040	79.960								
"			22.78	1.589	20.241	3.941	0.678	3.263	16.978	16.120	83.880								

$$^1 \text{I. - II. = III.; IV. - V. = VI.; III. - VI. = VII.; } \frac{\text{VI.} \times 100}{\text{III.}} = \text{VIII.; } \frac{\text{VII.} \times 100}{\text{III.}} = \text{IX.}$$

LARGE DOG.

SMALL DOG.



administration of the salt; and were Buchheim's theory correct, the maximum ought to be coincident with administration.

Of greater interest and practical value is the effect of the purgative on the quantity of the solids of the fæces, especially of the inorganic portion of them. This is best shown by the table on the opposite page, in which I have rectified the modification of the quantity of the solids produced by the presence of the purgative salt. In order to obtain the real weight of the total solids, I have deducted the weight of the estimated excess of crystalline sulphate of soda from the weight of the dried fæces; and to ascertain the real amount of the inorganic solids, the excess of the sulphate, reckoned as water-free, has been subtracted from the weight of the ash, as given in the previous tables. In each case, before drying and burning, the fæces were mixed with 2 grammes of dried carbonate of soda; but that, of course, has been deducted from the weights stated. For reasons already given I am inclined to attach more value to the analyses of the fæces of the large dog.

The most remarkable feature in the table is the considerable increase in the inorganic constituents of the fæces, apart from the purgative salt present. In the case of the large dog, after the administration both of the diluted purge and of the concentrated purge, but notably after the latter, is this increase evident, amounting to 1 gramme in the former experiment, and to more than 2 grammes in the latter. The organic matter, meanwhile, remains much as normally, or, if altered, it is diminished, as on the first day of Experiment XXV., where the diminution is probably due to the dog having had no forenoon meal, as always happened on the day of the administration of the salt. In the experiments with the small dog, whilst the same result is very observable, even for three days after the administration of the concentrated salt, yet the diluted salt is not followed by an increase in the proportion of the inorganic to the organic matter, although it causes a small absolute excess of the former. Setting aside this, the sole exceptional result, and for which it may be possible in part to account, as, for example, by the short time the salt remained in the canal, we may reasonably conclude that sulphate of soda and probably other saline purgatives increase the quantity of the inorganic constituents of the fæces, independent

of the salt which they themselves add. The quantity of the organic matter is, on the contrary, little affected. The increase of the organic matter in Experiment XXIII. is doubtless due to the salt acting on a previously constipated bowel.

What is the composition of this increase of the faecal ash, and what its source? I regret that, not having observed the increase while making the analyses, I did not examine its composition. It was not due to silica, for that I never found to exceed 5 per cent. of the total ash. It must, therefore, have consisted of salts soluble in water and acid.

The source of the increase of ash could not have been the food, as the same kind of bread was used when the normal ash was calculated as during the purgative experiments. The bread contained 42 per cent. of water, and 1.10 per cent. of ash, and each dog received 500 grammes per diem. The total ash in this quantity would not exceed 5.5 grammes, the greater part of which, consisting of soluble salts, must have been absorbed long before the food reached the rectum; and as defaecation was delayed in those experiments where the ash of the faeces attained its maximum, ample time was allowed for absorption. The source of the ash must, therefore, be the blood, through some form of intestinal secretion. Such a conclusion is inconsistent with Buchheim's theory, but I have already admitted that his theory is insufficient and possibly erroneous. In all probability an intestinal secretion, relatively rich in inorganic but poor in organic matter, is excited both by the diluted and by the concentrated salt. When the latter is administered, the water of the secretion is re-absorbed, whilst its solids, especially the ash, remain within the alimentary canal. To this point I shall return.

The three subsequent series of experiments will show how I was forced to abandon the view with which I commenced this inquiry into the action of saline cathartics. In these will be offered for the first time indisputable proof of the salt exciting secretion within the intestines. The nature of the secretion will likewise be discussed, and notice taken of some remarkable variations in quantity which the salt undergoes in its passage through the alimentary canal.

*(To be continued.)*

# THE BOSTON SOCIETY FOR MEDICAL OBSERVATION

## RESEARCHES ON THE GASES OF THE BILE. By J. J.

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ALTHOUGH the knowledge of the gases of the bile is of material interest in the criticism of many important questions, yet very few researches of any moment have been made in this direction.

Pflüger was the first who investigated this question, when, in 1869, he analysed nearly all the secretions, in order to obtain reliable data regarding the tensions of the respiratory gases in the tissues. In his discussion of this subject, Pflüger gives the two following analyses of the gases of dog's bile:—

EXPERIMENT I.			EXPERIMENT II.		
Amount of bile—13·066 c.cm.; reaction alkaline; food probably vegetable.			Amount of bile—23·8 c.cm.; reaction neutral, or faintly acid; food, animal.		
		Per cent.			Per cent.
Oxygen,		0·2	Oxygen,		0·00
Carbonic acid (by exhaustion),	14·4		Carbonic acid (by exhaustion),	5·00	
Carbonic acid (by phosphoric acid),	41·7		Carbonic acid (by phosphoric acid),	0·60	
Total,		56·1	Total,		5·60
Nitrogen,		0·4	Nitrogen,		0·6

The gases are at 0° C. and 1 metre pressure.

Pflüger's analyses showed that there were present in the bile only vanishing traces of oxygen and very little nitrogen—less even than that contained in the blood; whilst the largest proportion of the gases in the bile consisted of very variable quantities of free and combined carbonic acid. Why the carbonic acid varies so much in the dog's bile has not been explained; but it is to be noted that Pflüger's analyses refer to bile that has been in the gall-bladder, and not to bile flowing fresh from the liver itself. Now, it is probable there are great differences between the two kinds of bile; for, as is well known, the longer the bile

<sup>1</sup> E. Pflüger: "Die Gase der Secrete," *Arch. für die ges. Physiologie*, Bd. ii. p. 156.

remains in the gall-bladder the greater is the alteration, not only in its chemical constitution through the oxidation of its pigment, but also, to a considerable extent, in its density by absorption of its fluid constituents.

The next investigator was N. Bogoljubow,<sup>2</sup> who, at the instigation, and under the direction of Professor N. Kowalewsky, analysed the gases of the bile, but only with reference to the carbonic acid. He published ten analyses, of which eight were made on the bile of the gall-bladder of the dog, one upon the freshly-secreted bile of a dog narcotised with opium, and one on the bile from the gall-bladder of the sheep. In order to judge of his results, I annex the following table:—

Experiment.	Quantity of Bile in c.cm.	Free CO <sub>2</sub> in c.cm.	Combined CO <sub>2</sub> in c.cm.	Free CO <sub>2</sub> in 100 c.cm. of bile.	Combined CO <sub>2</sub> in 100 c.cm. of bile.	Total CO <sub>2</sub> in 100 c.cm. of bile.	Remarks.
1	6.649	1.298	2.468	19.5	37.0	55.5	Bile direct from liver; animal food.
2	9.975	1.708	6.237	17.1	62.5	79.6	Bright yellow, thin flowing, probably freshly-secreted bile from gall-bladder; animal food.
3	11.332	...	2.876	...	25.4	...	Do. do.
4	14.699	1.805	...	12.3	...	...	
5	10.386	1.089	0.251	10.5	2.4	12.9	
6	14.202	0.971	...	6.8	...	...	Green bile from gall-bladder; animal food.
7	17.522	0.387	...	5.06	...	...	Brown thin flowing bile from gall-bladder; animal food.
8	11.167	1.358		...	...	12.1	Bile from gall-bladder of a dog fed with bread.
9	9.549	0.302	0.028	3.16	0.29	3.45	Bile from gall-bladder of a fasting dog.
10	7.968	1.245	0.046	15.6	0.6	16.2	Green bile from gall-bladder of a sheep.

Unfortunately, Bogoljubow has forgotten to state to what pressure the gases were corrected, whether to a pressure of 1 metre or 760 mm. But, as up to that time, after the example of C. Ludwig and his school, 1 metre was generally taken as the

<sup>2</sup> N. Bogoljubow of Kasan: "Kohlensäuregehalt der Galle" in *Centralblatt für die Med. Wissenschaften*, 1869, No. 42, p. 657.

standard of comparison, we may safely regard this as applicable to Bogoljubow's numbers.

Bogoljubow's analyses, like those of Pflüger, show that the bile from the gall-bladder of the dog is sometimes extraordinarily rich in carbonic acid—especially in the combined form (see Exp. 2); but it sometimes also seems very poor in that constituent, as his analyses 5 and 6 indicate. Bogoljubow maintained that vegetable food furnished the least, and animal food the most, carbonic acid. He further believed that the carbonic acid in the bile was lessened by its stay in the gall-bladder. It may be seen that he obtained the least carbonic acid in the case of the fasting dog; but this, it is true, was only a single experiment.

In any case, however, Bogoljubow's experiments are insufficient to enable us to pronounce upon the causes of the differences between bile freshly secreted and bile from the gall-bladder. For in the nine analyses he made of the gases of the bile from the gall-bladder, such wide differences exist in the amount of carbonic acid that in one case there is as much as 79·6 per cent. (Exp. 2) and in another as little as 3·45 per cent. (Exp. 9), whilst in his only experiment with freshly-secreted bile the total carbonic acid amounted to 56·5 per cent. (Exp. 1). It must first be determined whether or not the freshly-secreted bile shows as great a variation in the amount of its carbonic acid as appears to be the case with bile from the gall-bladder.

The influence of the kind of the food also upon the amount of carbonic acid cannot be regarded as proved by the small number of Bogoljubow's communicated experiments. For only experiments, 2 and 8, of his table are of any value in enabling us to form an opinion on this question, whilst experiment 3 tells rather against his statement that animal food determines a high and vegetable food a low proportion of carbonic acid. Bogoljubow may have even been confirmed in this last view of his by the fact that the bile of the gall-bladder of the sheep furnishes a much smaller proportion of carbonic acid. Unfortunately, here again Bogoljubow has only supplied us with a single analysis of the gases of the bile; and, strange to say, *this is the only analysis we have up to the present of the gases of the bile of a herbivorous animal.*

The history of this interesting investigation may be brought to a close by quoting the following analysis by Noël<sup>1</sup> of dog's bile.

	Volume per cent.
Oxygen, . . . . .	1.22
Carbonic acid, . . . . .	4.03
Nitrogen, . . . . .	9.13

But this analysis cannot be regarded as of any value, as it is not stated at what pressure the gases were estimated;<sup>2</sup> besides, the amount of oxygen and nitrogen is certainly wrong; further, it is not stated how the carbonic acid was obtained, whether by exhaustion or by the addition of an acid; and finally, it is not indicated in any way whether the bile had been freshly secreted or had come from the gall-bladder.

We see, therefore, that numerous investigations are still necessary in order to clear up these important points.

This summer I undertook a series of experiments on this subject in the Physiological Laboratory at Bonn, and the following was the method I adopted:—

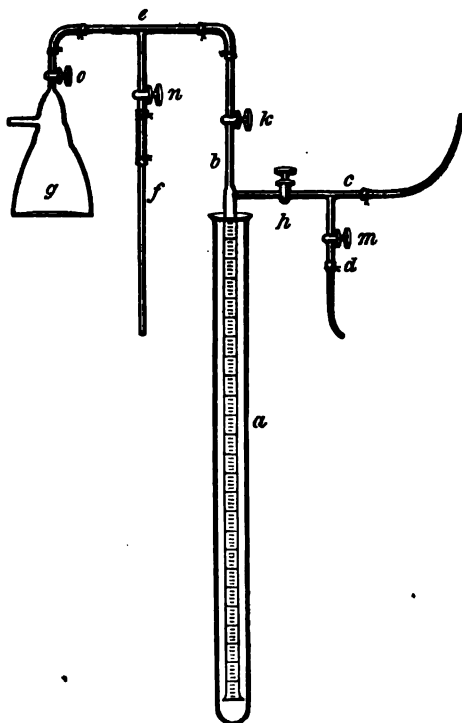
The animal was firmly fixed, and the abdomen being opened in the line of the *linea alba*, the common bile duct was sought for, and a small cannula fixed into it, and to this in turn an elastic tube was attached. The wound was then closed, and the projecting tube brought into air-tight connection with the glass apparatus next to be described.

The accompanying figure will render the arrangement of the apparatus easy to be understood: *a* is the collecting tube, which stands vertically in another wider tube containing mercury; it is 1.5 cm. wide, accurately calibrated, and divided into millimetres; *b* is the continuation of *a*, but of much narrower bore; *c* is a similar tube running horizontally, and fixed at right angles to *a* at the point where its diameter diminishes; *e* is a horizontal tube in connection at one end with *b*, and at the other with *g*, the receiver of Pflüger's pump. Both these horizontal tubes *c* and *e* are respectively provided with secondary

<sup>1</sup> G. Noël: "Étude générale sur les variations physiol. des gaz du sang," *Thèse*, Paris, 1876. Quoted by Hoppe-Seyler: *Physiologische Chemie*, ii. Theil, 1878, p. 307.

<sup>2</sup> Hoppe-Seyler, however, supposes that 760 mm. may be taken as the pressure.

tubes *d* and *f*, attached at right angles; *d*, although depicted as running down vertically from *c*, really runs horizontally; *h*, *k*, *m*, *n*, and *o* are Geissler's stop-cocks, whereby at pleasure more or less of the apparatus can be rendered perfectly air-tight. Before the experiment begins the whole apparatus is filled with mercury so completely, that not a trace of air exists even in the bore of the stop-cocks. The tube *f* has joined to it another fine tube, which opens freely into the air; and it is by the latter tube that at the required moment phosphoric acid makes its way into the receiver. The next preliminary operation is to exhaust the receiver, and at the same time the tube *e* as far as *k*, the stop-cocks *k* and *n* being closed and *o* open. The mercury now extends from the collecting tube *a* as far as the stop-cock *k*, whose bore, as well as the tubes *c* and *d*, it also fills. The stop-cocks *h* and *m* are then closed. When the experiment is to commence *m* is opened, and from the open end of *c* the mercury is blown out through *m* and the tube *d*. From the collecting tube *a* the mercury will therefore extend into *c* only so far as the point where the tube *d* is given off.



The elastic tube conveying the bile is now closely bound to the tube *c*, and sufficient time is allowed to elapse until the bile has begun to drop out of the tube *d*, and all the air has been expelled from *c* and *d* and replaced by bile. The stop-cock *m* must next be closed and *h* opened, and care must at the

same time be taken, by the regulated addition of washed mercury to the cylinder enclosing the collecting tube *a*, that no suction should occur from the falling of the mercury in the collecting tube. At the commencement of the experiment, but for a few seconds only, such a suction is very likely to occur; however, no unfortunate results follow, as it acts for only a few moments, and all the connections remain hermetically closed. In collecting larger quantities of bile one might be disposed to hasten the flow by suction; but this we found was not only of no advantage, but was even liable to produce lacerations of the fine biliary canals, extravasations of blood and lymph, entrance of air into the system of tubes conveying the bile, &c.; and therefore the only reliable plan to be followed, so as to approach as closely as possible to the normal conditions, is to allow the secretion of bile to go on of itself under little or no positive pressure. In this way we have generally arrived at extremely favourable results, and even obtained from the same animals very considerable volumes of bile, often, indeed, sufficient for several experiments.

As soon as a sufficient quantity of bile has been collected we close the cock *h*, and by means of the scale on the collecting tube *a* we read off accurately the volume of bile present. Then by a stroke of the pump we assure ourselves of the completeness of the vacuum as far as *k*. Next *k* is opened. The bile now rushes into the pump, and we allow it to continue to flow until the connecting tubes have been filled with mercury, and a small quantity of it has entered the receiver. The mercury, as it follows the bile, is intended to sweep this fluid as completely as possible from the tubes along which it has passed. The receiver is then immersed in a water-bath at 40° C., and the evacuation of the gases proceeds as in the analysis of serum-gas. The gases are here also evolved with difficulty on account of the slowness with which the sodium bicarbonate is decomposed. Zuntz,<sup>1</sup> by a series of exhaustions at 40° C. (extending over twelve hours, and carried out in Pflüger's laboratory) of a dilute solution of sodium sesquicarbonate, obtained about one-

<sup>1</sup> Zuntz: "Ueber den Einfluss des Partialdrucks der Kohlensäure auf die Vertheilung dieses Gases im Blute," *Centralblatt für die Medicinischen Wissenschaften*, 1867, No. 34.



third of the loosely combined carbonic acid. Then, even with the temperature raised to 60° C., he was compelled to continue the exhaustion for two days before the remaining nearly two-thirds were expelled. It therefore follows that it is a very difficult operation to obtain by exhaustion all the loosely combined carbonic acid present in sodium bicarbonate solutions. Accordingly, so soon as very small quantities only begin to come off, the tube *f* is immersed in a dilute well-boiled solution of phosphoric acid, the stop-cocks *n* and *o* are opened, and the acid allowed to enter in large excess. In this way, it is true, some of the atmospheric air absorbed by the phosphoric acid solution enters the receiver, and also a small portion at the open end of the tube *f*, yet this is of little consequence, as the gas liberated by the acid is collected in another receiver, and here at present we have only to deal with the carbonic acid. The evolution of this second portion of gas by the action of the acid is effected in a few minutes; all the gas is then in the free state, and the vacuum of the receiver is quickly restored. But here it should be noted that the receiver still contains an excess of phosphoric acid which is absolutely free of gas, and in sufficient amount to set free with a little shaking all the combined carbonic acid in a second supply of fresh bile.

If we make use of a large strong rabbit or dog, we do not remove the animal from the apparatus in which the bile is collecting whilst the first portion of the bile is being discharged. After this first quantity has made its way into the receiver, the stop-cocks *k* and *o* are closed and *k* again opened. The bile, which meanwhile had almost come to rest, now runs rapidly again into the collecting tube *a*; and often the experiment is so far successful that sufficient bile is obtained for a second analysis, if the precaution be taken to squirt water from time to time into the mouth of the animal, so that it may swallow as much as it desires. Rabbits, on account of their relatively energetic biliary secretion, lose a very considerable proportion of water in their very thin liver secretion. If this second quantity of bile be now admitted to the receiver with the same precautions as the first portion, it only remains necessary to observe whether the phosphoric acid in the receiver is still present in excess. At the conclusion of the second experiment, we should always assure ourselves of the strong acid reaction of the fluid in the receiver.

With respect to the gas analysis, it only remains for me to make a few additional observations. I have, for example, estimated the oxygen and nitrogen collectively only, and this demands an explanation. Pflüger, in his treatise on the gases of the secretions, was the first to point out that in every secretion he examined, with the exception of the saliva,<sup>1</sup> there was no oxygen present, or only a trace.<sup>2</sup> These facts discovered by Pflüger have been confirmed by Hoppe-Seyler, who, in an ingenious manner, has taken advantage of the spectroscopic properties of hæmoglobin for the determination of oxygen. He has thus furnished evidence of the absence, or at least of the presence of mere traces, of free oxygen in the *freshly-secreted* bile—a fact of more importance in our present investigations, as Pflüger's analyses dealt only with bile from the gall-bladder, and cannot therefore be applied with strict accuracy to the bile flowing directly from the liver. F. Hoppe-Seyler says<sup>3</sup>:—"I have convinced myself (*Zeitschrift f. Physiol. Chemie.* i. p. 137), in the case of the dog, that during digestion the freshly-secreted bile is generally either free from absorbed oxygen, or that the amount present is at least less than 0.15 per cent. by volume. Hæmoglobin solutions mixed with such bile, in the absence of air, does not give with the spectroscope the two absorption bands of oxyhæmoglobin."

Accordingly, the gases contained in the bile consist of carbonic acid and nitrogen, oxygen being absent, or present only in traces whose quantity is so small that they may be estimated as among the errors of experiment. I may, therefore, be allowed to regard the collective percentage estimation of the nitrogen and oxygen as sufficient for our purpose.

The methods of gas analysis employed were those of Professor Bunsen.

#### I. *Experiments on the Gases of the Bile of the Rabbit.*

It appeared indispensable to me, in the first place, to make myself acquainted with the composition of the gases in the bile of the herbivora. Accordingly, for this purpose I made use of the rabbit. From this animal it was easily possible, after the

<sup>1</sup> E. Pflüger, "Die Gase des Speichels," *Pflüger's Archiv*, i. p. 686.

<sup>2</sup> E. Pflüger, "Die Gase der Secrete," *Pflüger's Archiv*, ii. p. 176.

<sup>3</sup> Felix Hoppe-Seyler, *Physiologische Chemie*, ii. p. 307.

establishment of a temporary biliary fistula with exclusion of air, to obtain sufficient freshly-secreted bile for two analyses, if the rabbit was at least from 1500 to 2000 grms. in weight.

*Experiment 1.*—Rabbit 870 grms. ; 16 c.cm. of alkaline bile. <sup>1</sup>

	Volume.	Pressure.	Temperature.	Volume 0° C. and 1 metre pressure.
	c.cm.	m.	deg.	c.cm.
Free carbonic acid, oxygen, and nitrogen, . . . .	12·69	0·2504	22·8	2·933
After absorption of the carbonic acid, . . . .	1·16	0·2084	20·0	0·22373
Combined carbonic acid, . .	43·54	0·4037	22·8	16·228
After absorption, . . . .	6·33	0·2119	22·0	1·224

After correction for temperature (0° C.) and pressure of:—

	1 Metre.	760 mm.
	Per cent.	Per cent.
Free carbonic acid, . . . .	16·94	22·3
Carbonic acid liberated by phosphoric acid,	93·69	123·2
Total carbonic acid, . . . .	110·62	145·2
Nitrogen and oxygen, . . . .	1·4	1·8

*Experiment 2.*—Rabbit 2100 grms. ; 20·5 c.cm. of alkaline bile.

	Volume.	Pressure.	Temperature.	Volume 0° C. and 1 metre pressure.
	c.cm.	m.	deg.	c.cm.
Free carbonic acid, oxygen, and nitrogen, . . . .	11·87	0·2334	23·25	2·554
After absorption of the carbonic acid only a very small residue. Number lost from cracking of collecting tube ; about 0·02 c.cm. of oxygen and nitrogen.				
Combined carbonic acid, . .	50·492	0·4173	23·25	19·41
After absorption, . . . .	3·92	0·2219	22·5	0·804

<sup>1</sup> The specific gravity was not taken, partly in consequence of the extreme difficulty of doing so conveniently during the course of the experiment, and partly on account of its rapid variation and diminution as the bile flows from the liver.

After correction for temperature (0° C.) and pressure of:—

	1 Metre.	760 mm.
	Per cent.	Per cent.
Free carbonic acid, . . .	11·55	15·2
Combined carbonic acid, . . .	90·82	119·5
Total carbonic acid, . . .	102·37	134·7
Nitrogen and oxygen, . . .	0·98	1·3

*Experiment 3.*—The same rabbit, which, after the first quantity of bile had been admitted into the receiver, emptied its bile still further into tube *c* for this experiment. By this means the whole of the gases were obtained independently at once, the phosphoric acid being present in excess in the receiver. Quantity of bile = 9·5 c.cm.

	Volume.	Pressure.	Temperature.	Volume corrected to 0° C. and 1 metre.
	c.cm.	m.	deg.	c.cm.
Combined and free carbonic acid, . . .	41·13	0·274	23·25	10·397
After absorption, . . .	1·38	0·1067	22·5	0·136

After correction for temperature (0° C.) and pressure of:—

	1 Metre.	760 mm.
	Per cent.	Per cent.
Total carbonic acid, . . .	108·01	142·1
Nitrogen and oxygen, . . .	1·43	1·9

*Experiment 4.*—Rabbit 2280 grms.; 25·0 c.cm. of alkaline bile.

	Volume.	Pressure.	Temperature.	Volume at 0° C. and 1 metre.
	c.cm.	m.	deg.	c.cm.
Free carbonic acid, oxygen, and nitrogen, . . .	12·56	0·2396	23·0	2·7758
After absorption of carbonic acid, . . .	1·94	0·1974	24·1	0·3405
Combined carbonic acid, . . .	69·669	0·424	23·0	27·21
After absorption, . . .	7·946	0·125	24·1	0·9142

After correction for temperature (0° C.) and pressure of:—

	1 Metre.	760 mm.
	Per cent.	Per cent.
Free carbonic acid, . . .	9.75	12.8
Combined carbonic acid, . . .	105.18	138.4
Total carbonic acid, . . .	114.9	151.2
Oxygen and nitrogen, . . .	1.36	1.8

*Experiment 5.*—The same rabbit, which this time furnished 10.5 c.cm. of bile.

	Volume.	Pressure.	Temperature.	Volume at 0° C. and 1 metre.
	c.cm.	m.	deg.	c.cm.
Total gas, . . .	38.22	0.3361	23.0	11.848
After absorption of the carbonic acid, . . .	0.73	0.1857	24.1	0.1244

After correction for temperature (0° C.) and pressure of:—

	1 Metre.	760 mm.
	Per cent.	Per cent.
Total carbonic acid, . . .	111.66	146.9
Oxygen and nitrogen, . . .	1.19	1.4

As a general result of the preceding analyses it appears that the bile of the rabbit is extraordinarily rich in carbonic acid, *more so indeed than any other of the animal fluids yet examined.* This result was confirmed by an alkalimetric estimation of rabbit's bile kindly made by Dr. Antweiler, chemical assistant in the Physiological Institute of Bonn. He found 100 c.cm. of rabbit's bile capable of fixing 106 c.cm. of carbonic acid (at 0° C. and 1 m.) in the form of bicarbonate, a result which is in the closest correspondence with the above analyses. Bogoljubow's statement, therefore, that the bile of the sheep contains only a total of 16.2 per cent. of carbonic acid must therefore be regarded as requiring further proof.

## II. *Experiments on the Gases of the Bile of the Dog.*

The methods were the same as those employed in the case of the rabbit.

*Experiment 6.*—Dog of 11·400 grms. Not narcotised. Weakly alkaline bile=20·5 c.cm. The dog had been well-fed for three days previously on horses' flesh.

	Volume.	Pressure.	Temperature.	Volume at 0° C. and 1 metre.
	c. cm.	m.	deg.	c. cm.
Total free gas, . . .	22·66	0·1796	22·0	3·765
After absorption of the carbonic acid, . . .	2·79	0·1023	22·4	0·2639
Combined carbonic acid, . .	22·361	0·2966	22·0	6·139
After absorption, . . .	·635	0·172	22·4	0·10124

After correction for temperature (0° C.) and pressure of:—

	1 Metre.	760 mm.
	Per cent.	Per cent.
Free carbonic acid, . . . .	17·1	22·5
Combined carbonic acid, . . . .	29·45	38·7
Total carbonic acid, . . . .	46·55	61·2
Nitrogen and oxygen, . . . .	1·29	1·7

*Experiment 7.*—A strong dog of 12 kilogrammes furnished in the first place 22 c.cm. of alkaline bile. The beast was strongly narcotised with morphia. The 22 c.cm. flowed away in fifty minutes.

	Volume.	Pressure.	Temperature.	Volume at 0° C. and 1 metre.
	c. cm.	m.	deg.	c. cm.
Free carbonic acid, oxygen, and nitrogen, . . .	15·54	0·2472	25·0	3·5185
After absorption of the carbonic acid, . . .	2·12	0·1965	24·5	0·3823
Combined carbonic acid, . .	34·33	0·3224	25·0	10·13
After absorption, . . .	3·72	0·202	24·5	0·6896

After correction for temperature (0° C.) and pressure of:—

	1 Metre.	760 mm.
	Per cent.	Per cent.
Free carbonic acid, . . . .	14·28	18·8
Combined carbonic acid, . . . .	42·96	56·4
Total carbonic acid, . . . .	57·24	75·3
Oxygen and nitrogen, . . . .	1·74	2·2

*Experiment 8.*—The same animal furnished 20 c.cm. more bile, which was allowed to flow into the excess of phosphoric acid already present in the pump.

	Volume.	Pressure.	Temperature.	Volume at 0° C. and 1 metre.
	c.cm.	m.		c.cm.
Free and combined carbonic acid, . . . .	60·37	0·3659	25·0°	20·228
After absorption, . . . .	2·26	0·935	24·5°	19·40

After correction for temperature (0° C.) and pressure of:—

	1 Metre.	760 mm.
	Per cent.	Per cent.
Total carbonic acid, . . . .	100·15	131·8
Oxygen and nitrogen, . . . .	0·970	1·2

Here we may ask ourselves whether this high proportion of carbonic acid may not be the result of the strong morphia narcosis into which the animal was brought by the repeated morphia injections.<sup>1</sup>

We shall now collect the essential results of the previous experiments in the following table:—

<sup>1</sup> Morphia increases the quantity of carbonic acid in the blood; this may possibly explain the above.

*General Table of the Experiments upon the Gases of the Bile of the Rabbit.*

The gases are at 1 metre pressure.

No. of Experiment.	Free Carbonic Acid.	Carbonic Acid liberated by the action of an acid.	Total Carbonic Acid.
	Per cent.	Per cent.	Per cent.
1	16.94	93.69	110.62
2	11.55	90.82	102.37
3	...	...	108.01
4	9.75	105.18	114.9
5	...	...	111.66
			Total, 547.56
			Mean, 109.5

*General Table of the Experiments upon the Gases of the Bile of the Dog.*

No. of Experiment.	Free Carbonic Acid.	Carbonic Acid liberated by the action of an acid.	Total Carbonic Acid.
	Per cent.	Per cent.	Per cent.
1	17.1	29.45	46.55
2	14.28	42.96	57.24
3	...	...	100.15

The small number of experiments on dogs, and the extraordinarily high value obtained in experiment 3, where there was such deep narcosis, a value, too, differing so much from the others, forbids us drawing a mean in the case of these experiments. But it is worthy of remark that the numbers Bogoljubow obtained in an analysis of freshly secreted bile corresponds very closely with our experiment 2, and indicates 56.5 per cent. These experiments are therefore not unfavourable to Bogoljubow's view that the bile direct from the liver is generally richer in carbonic acid than that from the gall-bladder. For whilst both Pflüger and Bogoljubow in experimenting with the bile from the gall-bladder obtained a comparatively small proportion of carbonic acid, this was never found to be the case in any experiments on the freshly secreted bile.



If we compare the amount of carbonic acid present in the freshly secreted bile of the dog with that of the herbivorous rabbit, we are struck at once with the much higher proportion in the case of the latter. This, as has been established by Dr. Antweiler's alkalimetric determination, is due to the extremely high proportion of carbonated alkali present, and shows what large quantities of alkaline fluid are poured out into the small intestine of herbivorous animals. *No fluid in the body is as yet known which indicates such a high proportion of carbonated alkali.* On one occasion Pflüger<sup>1</sup> obtained a total amount of 64·7 per cent. of carbonic acid in the saliva of the dog; this number is calculated at a pressure of 1 metre. But whether a still higher proportion is present in the saliva of the herbivora, has unfortunately not yet been determined.

As to the question of the cause of such a great difference in the amount of carbonic acid in the bile of carnivora and herbivora, a decisive answer cannot be given from our present knowledge of the subject. We may, however, regard as settled that this difference is dependent on the different and varying proportion of carbonated alkali present; and this may possibly indicate that the blood from which the bile is separated is probably richer in carbonated alkali in herbivora than in carnivora.

Unfortunately we know so very little about the quantitative proportions of the gases in the blood of herbivora that we can with safety found no satisfactory hypothesis as to the amount of carbonic acid upon the alkalinity of their blood. In C. Ludwig's<sup>2</sup> laboratory some analyses, after the old-fashioned imperfect exhaustion-methods, were performed by Wilhelm Preyer<sup>3</sup> with sheep's blood, in which, however, no high proportion of carbonic acid was obtained. It amounted respectively in four analyses of the blood of the sheep to 35·75, 35·66, 38·01, and 37·53. According to Pflüger's researches<sup>4</sup> normal arterial dog's

<sup>1</sup> E. Pflüger: "Die Gase des Speichels," *Pflüger's Archiv*, i. p. 688.

<sup>2</sup> C. Ludwig: "Zusammenstellung der Untersuchungen über Blutgase," *Zeitschrift der k. k. Gesellschaft d. Aerzte in Wien*, 1865, p. 8.

<sup>3</sup> W. Preyer: "Ueber die Bindung und Ausscheidung der Blutkohlenensäure bei der Lungen- und Gewebeathmung," *Sitzungsberichte der kaiserl. Akademie der Wissenschaften*, Bd. xlix., Sitzung vom 8 Jänner.

<sup>4</sup> E. Pflüger: "Die normalen Gasmengen des arteriellen Blutes nach verbesserten Methoden," *Centralblatt für die Medicinischen Wissenschaften*, 1867, No. 46.

blood contains 26·2 per cent. of carbonic acid, and the venous therefore, in which 5 to 6 per cent. more exists—almost as much as the venous blood of the sheep, if dependence is to be placed on Preyer's analyses. Pflüger observed in one case<sup>1</sup> in the venous blood of the sheep, 50·0 per cent. of carbonic acid, and in another case 40·18 per cent. These comparatively few data render it at least probable that the blood of the herbivora is more alkaline than that of the carnivora. A more conclusive decision can only be obtained by such experiments as those made by Zuntz<sup>2</sup> in Pflüger's laboratory, in which alkalimetric blood-filtrations were performed.

Zuntz's very important work shows the difficulties which such analyses present on account of the great variability in the alkalinity of fresh blood. His results, however, afford at least very valuable indications for the question here at issue. Bogoljubow<sup>3</sup> concluded that such large quantities of carbonic acid as he had obtained in his first three experiments must depend on an energetic formation of carbonic acid in the liver; and this conclusion of his is, at any rate, not without some foundation. Zuntz<sup>4</sup> found that 100 c.cm. of fresh unaltered—that is, living, pig's blood, by virtue of its alkalinity, could fix 105 c.cm. of carbonic acid (at 0° C. and 1 m.) in the form of bicarbonate. In the living blood of the dog the values were 90 and 93 volumes per cent.

If we now take into consideration the extremely slow secretion-stream in the liver cells, and their probably not insignificant carbonic acid separation, we may fairly suppose that the alkaline combinations diffused from the blood into the liver cells find time and opportunity to bring away with them large quantities of carbonic acid, probably after effecting its separation, so as thus to occasion the large proportion of carbonic acid noted in the freshly formed secretion.<sup>5</sup>

<sup>1</sup> E. Pflüger: "Ueber die Kohlensäure des Blutes," Bonn, 1864, p. 7 u.f.

<sup>2</sup> N. Zuntz, "Beiträge zur Physiologie des Blutes," *Inauguraldissertation*, Bonn, 1868.

<sup>3</sup> Bogoljubow, *loc. cit.* p. 659.

<sup>4</sup> N. Zuntz, *loc. cit.* p. 4.

<sup>5</sup> It is perhaps possible that some of the carbonic acid excreted by the liver has been formed by the metamorphosis of albumen. Thus, a molecule of albumen might be supposed to combine with 50 molecules of water and to split up

From what has been said, it may be inferred that the experimental data for an investigation of the supposed seats of secretion-formation have yet to be found here. I must, however, restrict myself to these general remarks.

I cannot conclude without gratefully acknowledging the friendly guidance of Professor Pflüger, director of the Physiological Institute, Bonn; and at the same time I have to thank Dr. W. Kochs, Physiological Assistant, for his valuable aid.

in the liver into 8 molecules of urea, 7 of glycogen, 5 of carbonic acid, 7 of free oxygen, and 1 of sulphuric acid. The oxygen of course would not remain free, but might at once enter into other combination or even into the coloured corpuscles of the blood.

## Anatomical Notices.

### THE DEVELOPMENT OF THE MAMMARY GLAND.

By G. REIN.

(*Proceedings International Medical Congress, 1881.*)

The following six stages are to be distinguished in the development of the mammary gland:—

1. *Tubercular Stage*.—A little mass of epithelial cells, originating in a localised overgrowth of the cylinder-cells of the epidermis.

2. *Lenticular Stage*.—The under surface of the epithelial mass becomes convex, and penetrates into the cutis. The round embryonal elements of the latter are at this point collected in larger numbers, and form a zone from which the tissue of the nipple is subsequently developed (nipple-zone).

3. *Conical Stage*.—This results from the second stage by a further penetration of the epithelial elements into the cutis, with gradual flattening above, and simultaneous increase in height of the epithelial deposit.

4. *Club-shaped Stage*.—The epithelial deposit continues to grow and to make its way into the cutis in this stage. The deposit becomes so shaped that two parts can be distinguished, viz., a deeper globular part (body) and a superficial narrow, more or less elongated, part (neck).

5. *Formation of Buds*.—The buds appear in the epithelial deposit at first as little elevations, which are rapidly converted into long, originally solid plugs (secondary epithelial deposit). These plugs are the rudiments of the future epithelial constituents of the gland: sinus lactei, ductus lactiferi, and acini. In the cutis, beneath the nipple-zone, there becomes differentiated a new zone of younger, rounded, closely-packed connective tissue cells; from these is developed the stroma of the gland (stroma-zone).

6. *Degeneration of the Primary; Epithelial Deposit further Development of the Secondary*.—The retrogressive metamorphosis of the central cells of the primary epithelial deposit proceeds rapidly in this stage, and in many animals, as well as in man, assumes the character of a cornification. The cornification spreads from above and the centre downwards and to the periphery. As a result of these processes a shrinking of the primary epithelial deposit takes place, leading to its complete disappearance, which, however, is observed only in the course of extra-uterine life, and even then usually very late. Other constituents of the rudimentary gland—the epithelial plugs, nipple-zone, and stroma zone—become further developed in the sixth stage. The originally solid epithelial plugs become hollowed and converted into tubes; they give off tolerably numerous club-shaped buds at their ends. The acini only appear, as is already known, later, at the time of puberty.

NOTE ON ABNORMAL DISTRIBUTION OF THE THORACIC DUCT. By J. MACDONALD BROWN, M.B., *lately Demonstrator of Anatomy in the Owens College, Manchester.*

ALTHOUGH in the vast majority of cases, the thoracic duct holds practically identical relations, and terminates by opening into the left cervico-brachial veins, yet we occasionally find that it possesses an abnormal distribution. Cruickshank<sup>1</sup> describes a case where the duct was double in its entire extent, so that in this instance there was a right and a left thoracic duct each receiving the lymphatics of its own side. The same anatomist found it once to be triple or nearly so.

The most common deviation from the normal, however, is when it terminates in the right veins instead of the left. Cruickshank, Meckel,<sup>2</sup> Fyfe,<sup>3</sup> Fleischmann,<sup>4</sup> Todd,<sup>5</sup> Allen Thomson,<sup>6</sup> and Morrison Watson<sup>7</sup> have recorded cases very similar to the one which came under my notice in the Anatomical Rooms of the Owens College during the past session, and of which this is a brief notice.

These have been divided by Watson into two classes, viz., those accompanied by vascular peculiarities, and those in which the vessels were normal. The cases of Todd and Allen Thomson belong to the former class, the others to the latter. My own case was that of a female subject, in which the condition of the right cervico-brachial arteries was considerably modified. There was no innominate artery, but the right common carotid occupied its relations, the latter trunk being otherwise as on the left side. The right subclavian came off from the posterior aspect of the second part of the aortic arch, and passed upwards to the right side of the neck, lying behind the arch, trachea, and œsophagus. It was crossed by the thoracic duct, right vagus, and some cardiac nerves, and after it had emerged from behind the œsophagus, the right innominate vein lay in front of it, in the remainder of its intra-thoracic extent.

A similar distribution of the right subclavian artery has been noted by Turner, Hyrtl, Thomson, Henle, Spence, Struthers, &c.

*Thoracic Duct.*—There was no dilatation at its commencement as described by Pecquet, but it took its origin in a fine plexus of lymphatics opposite the first and second lumbar vertebrae, and in front of the column. The duct passed through the aortic opening of the diaphragm, and had the ordinary relations as high as the third dorsal vertebra, except that it was rather nearer the thoracic aorta than usual, in fact just over the origins of the right intercostal arteries. Opposite the 3d dorsal vertebra it inclined towards the right side, and passed into the right neck. It lay behind the superior vena cava, and right innominate vein, and rested successively upon the vertebral column, longus colli muscle, subclavian artery, vertebral

<sup>1</sup> *Anatomy of the Absorbing Vessels.*

<sup>2</sup> *Compendium of Anatomy,*

<sup>3</sup> *Todd's Cyc. Anat. Phys.* vol. iii.

<sup>7</sup> *Journ. Anal. and Phys.* vol. vi.

<sup>5</sup> *Diss. Epist. ad Haller.*

<sup>4</sup> *Leichenöffnungen.*

<sup>6</sup> *Quain's Anat.* vol. i. 8th ed.

vein, scalenus anticus muscle, and phrenic nerve. It passed up into the neck as high as the 6th cervical vertebra, then curving downwards and outwards it opened into the right subclavian vein about six lines external to its junction with the internal jugular. Its intrathoracic portion received three branches from the left thorax, joining it opposite the 10th, 11th, and 12th dorsal vertebræ respectively. The right lymphatic duct had its normal position, and opened *independently* into the retrocession of the veins. Upon the left side the lymphatic duct was present, and had similar relations and proportions to those which in normal conditions are to be found on the right side. This presents a marked difference from the cases described by Fyfe and Watson, in which no left lymphatic duct was found; while it agrees with the statement made by Cruickshank, that when the thoracic duct opens into the right veins, "then the trunk of the absorbents on the left side in every respect resembles the ordinary appearance of that of the right side."

In none of the cases previously cited is the right lymphatic duct described as opening separately into the venous channel as above. The arterial peculiarity found by Allen Thomson was that of a right aortic arch, while in Todd's case, there existed a vascular condition similar to the one which I have described.

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#### TRANSPPOSITION OF AORTA AND PULMONARY ARTERY.

By J. HALLIDAY SCOTT, M.D., *Professor of Anatomy, University of Otago.*

THE child from which this heart was taken was born alive, and presented no peculiarities on external examination.

The heart was of normal size. The shape was also normal, with the exception that the apex was formed by the right ventricle. The venæ cavæ and pulmonary veins opened in the normal manner into the right and left auricles respectively. The aorta and pulmonary artery, however, presented the unusual abnormality of arising from the wrong ventricles, the aorta springing from the right and the pulmonary artery from the left. The origin of the aorta thus lay in front and to the right of the pulmonary artery, and the left coronary artery passed in front of it. The ductus arteriosus was widely open. In other respects these vessels were normal, the aorta gave off its branches in the usual manner, and had a normal left-sided arch. The pulmonary artery divided as usual into two branches, one for the right and one for the left lung.

On examining the interior of the heart, the foramen ovale was found to be open to the extent of one-fourth of an inch at its anterior and upper part. There also existed a deficiency in the interventricular wall, rounded in form and about three lines across. It was surrounded by muscular fibre, and was situated about one-third of an inch below the right auriculo-ventricular opening, towards the back part of the septum.

**METASTATIC EXCRETION OF URINE.** By CHARLES A. CAMERON, M.D., *Fellow and Professor of Chemistry and Hygiene, Royal College of Surgeons, Ireland.*

DR. A. O. SPEEDY has at present under his care a young woman affected with hysteria and occasionally suffering from retention of urine. On several occasions a liquid was discharged from her breasts. On examining a specimen of this liquid, I obtained the following results:—Specific gravity, 1012; colour, pale yellowish-brown; reaction, faintly acid; urea, 2.1 per cent.; sodium chloride, 0.43 per cent. No fat or sugar was present, but the fluid contained a small quantity of albumin; the liquid had a faint urinous odour.

**RESEARCHES ON THE MINUTE STRUCTURE OF THE SPINAL CORD.** By Dr. GIAMBATTISTA LAURA, *Turin.*

(*Proc. International Medical Congress, 1881.*)

1. Many of the cells of the anterior cornu send their processes to the anterior nerve-roots; in the cervical region to the roots of the accessory nerve.

2. The anterior commissure receives fibres from the cells (a) of the various points of the anterior cornu, (b) of the various points of the posterior cornu.

3. The cells of Clarke's column are provided with processes which are directed at first inwards towards the central canal, but after a certain course they turn outwards in a very large fasciculus, which passes through the anterior cornu to the lateral white column. The group of cells, which in the *conus medullaris* occupies the position of Clarke's column, is not to be considered as its continuation, since its cells send their processes in a different direction, viz., outwards and forwards, to the anterior roots.

4. The white lateral column receives processes from the cells of many regions of the anterior and posterior cornua.

5. The cells of the posterior cornua are provided with processes, the direction of which presents a great variety. The following are noteworthy:—(a) cells sending processes forwards through the anterior cornu to the proximity of the anterior roots; (b) cells sending processes into the posterior white column; (c) cells sending processes across the middle line, and behind the central canal, to the opposite side of the medulla.

**ON THE SITUATION OF THE STOMACH, AND THE RELATION WHICH EXISTS BETWEEN ITS FORM AND ITS FUNCTIONS.** By Dr. LESSHAFT, *St. Petersburg.*

(*Proc. International Medical Congress, 1881.*)

THE results of the examination of 1200 subjects may be embodied in the following propositions:—

1. The stomach is placed vertically in the abdominal cavity, so that its fundus touches the diaphragm; its pyloric extremity is to the right; the lesser curvature is also to the right, with the upper part slightly inclined downwards; the great curvature is to the left.

2. The stomach is in the left hypochondriac region, and exclusively in the gastric region proper. The pylorus corresponds to a line drawn vertically downwards from the right border of the sternum.

3. The stomach, in consequence of its intimate relation with the neighbouring organs, cannot dilate by the displacement of one of its parts (the great curvature, for example); it can only become equally distended in all its parts in proportion to the accumulation of its contents.

4. The muscular coat of the stomach is made up of longitudinal, transverse, and oblique fibres. The longitudinal fibres are directed from the fundus towards the pylorus, and the circular fibres become thicker as they approach the latter; they are especially noticeable in the neighbourhood of the pylorus, where they form a true sphincter. It follows that the food-stuffs move in relation with the walls towards the pylorus, and this permits of a very advantageous admixture with the gastric juice. Then the contents return through the middle of the stomach towards the fundus, as there is here less resistance, on account of its larger size.

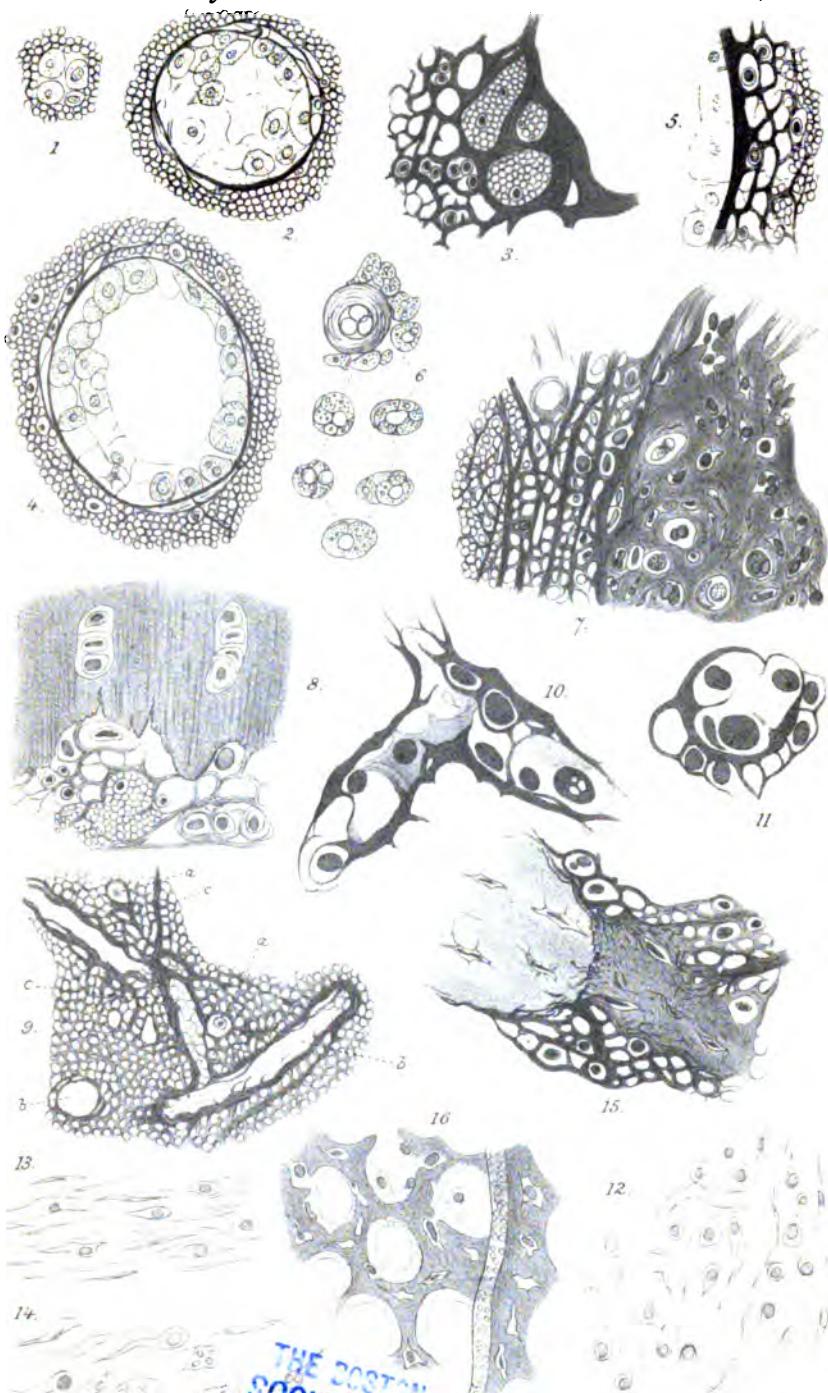
5. The peripheral movement of the contents of the stomach from the fundus towards the pylorus, and the central movement in the opposite direction, can only take place in consequence of the special shape of the stomach. The absence of the fundus in a new-born child, and its after development, can be accounted for by taking into consideration the influence of the returning middle column of digesting food upon the walls of the stomach. With regard to the lengthened stay of the food in the stomach, and the slowness of its after passage through the small intestines, it can be explained partly by the shape of the stomach and partly by the disposition of its muscular fibres.

6. In consequence of the accumulation of gas in the transverse colon, this rises in the shape of an arch, and is directed towards the left of the stomach, reaching to the level of the fourth intercostal space, and sometimes even to the fourth rib. If the gaseous accumulation takes place in the small intestine, this becomes displaced in the same way, and rises behind the transverse colon; the lower part of the stomach thus becomes slightly displaced anteriorly, and its direction is more oblique, from above downwards, and behind forwards.

7. A large stomach, accustomed to a generous food supply, always retains its vertical position, and it is only the pyloric extremity which is directed upwards and to the right.

8. The spleen is normally placed behind the upper part of the large curvature of the stomach; its direction corresponds to the tenth rib on the left side, or to the ninth intercostal space, from the point where this space is crossed by the axillary line. By its upper border it almost reaches the left side of the bodies of the vertebræ.





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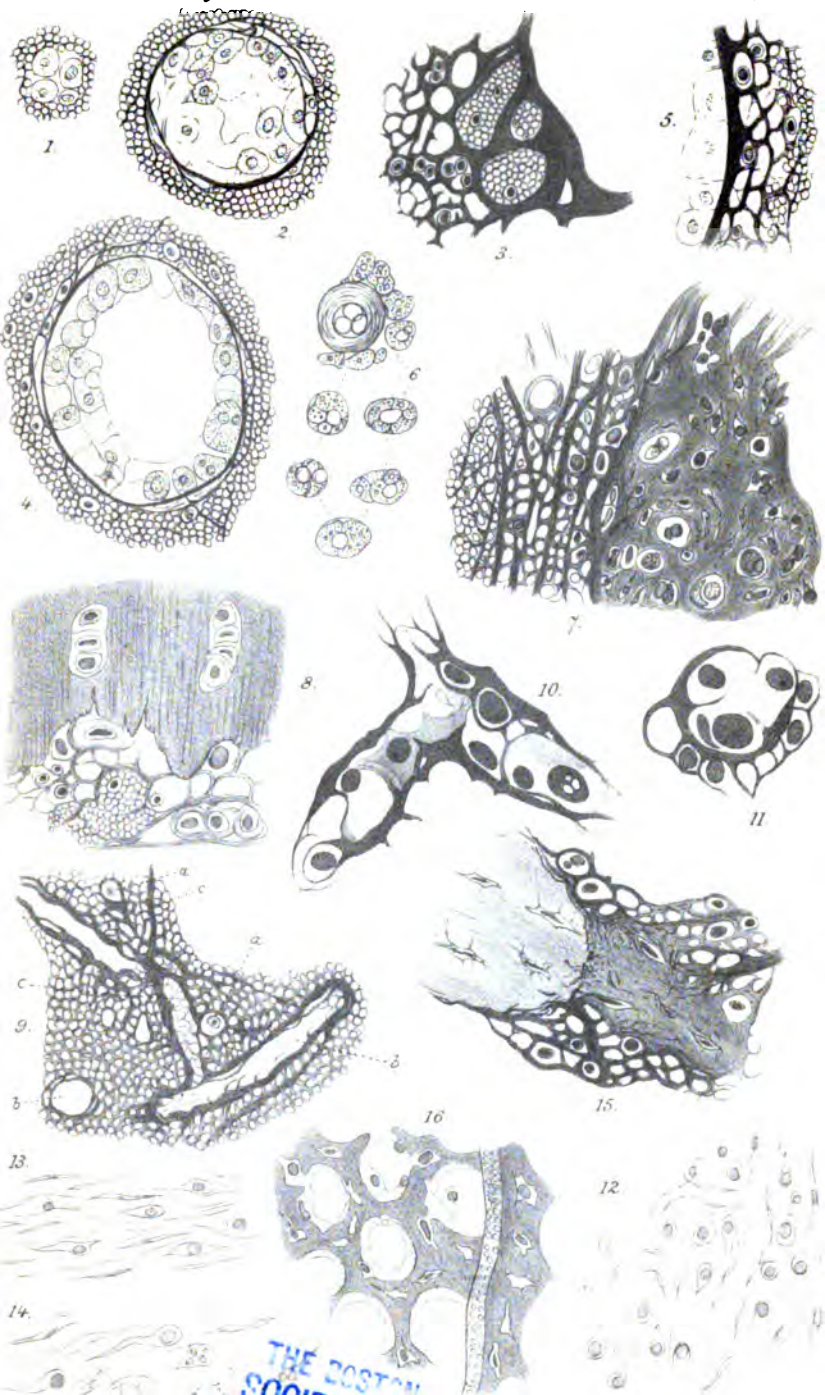
4. The muscular coat of the stomach is made up of longitudinal, transverse, and oblique fibres. The longitudinal fibres are directed from the fundus towards the pylorus, and the circular fibres become thicker as they approach the latter; they are especially noticeable in the neighbourhood of the pylorus, where they form a true sphincter. It follows that the food-stuffs move in relation with the walls towards the pylorus, and this permits of a very advantageous admixture with the gastric juice. Then the contents return through the middle of the stomach towards the fundus, as there is here less resistance, on account of its larger size.

5. The peripheral movement of the contents of the stomach from the fundus towards the pylorus, and the central movement in the opposite direction, can only take place in consequence of the special shape of the stomach. The absence of the fundus in a new-born child, and its after development, can be accounted for by taking into consideration the influence of the returning middle column of digesting food upon the walls of the stomach. With regard to the lengthened stay of the food in the stomach, and the slowness of its after passage through the small intestines, it can be explained partly by the shape of the stomach and partly by the disposition of its muscular fibres.

6. In consequence of the accumulation of gas in the transverse colon, this rises in the shape of an arch, and is directed towards the left of the stomach, reaching to the level of the fourth intercostal space, and sometimes even to the fourth rib. If the gaseous accumulation takes place in the small intestine, this becomes displaced in the same way, and rises behind the transverse colon; the lower part of the stomach thus becomes slightly displaced anteriorly, and its direction is more oblique, from above downwards, and behind forwards.

7. A large stomach, accustomed to a generous food supply, always retains its vertical position, and it is only the pyloric extremity which is directed upwards and to the right.

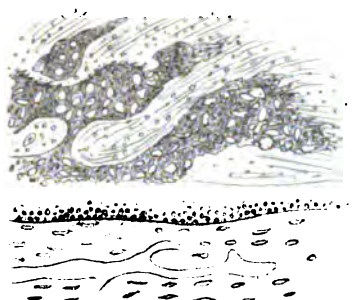
8. The spleen is normally placed behind the upper part of the large curvature of the stomach; its direction corresponds to the tenth rib on the left side, or to the ninth intercostal space, from the point where this space is crossed by the axillary line. By its upper border it almost reaches the left side of the bodies of the vertebræ.



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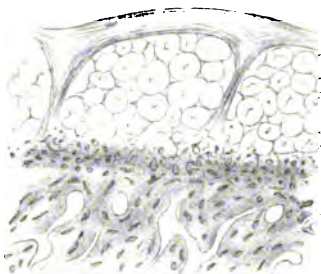
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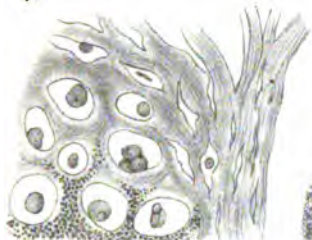
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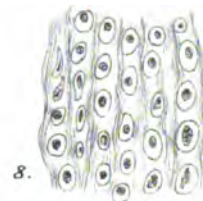
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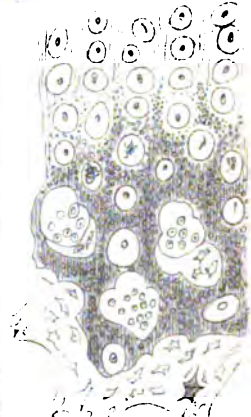
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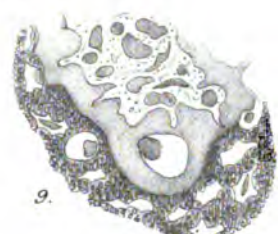
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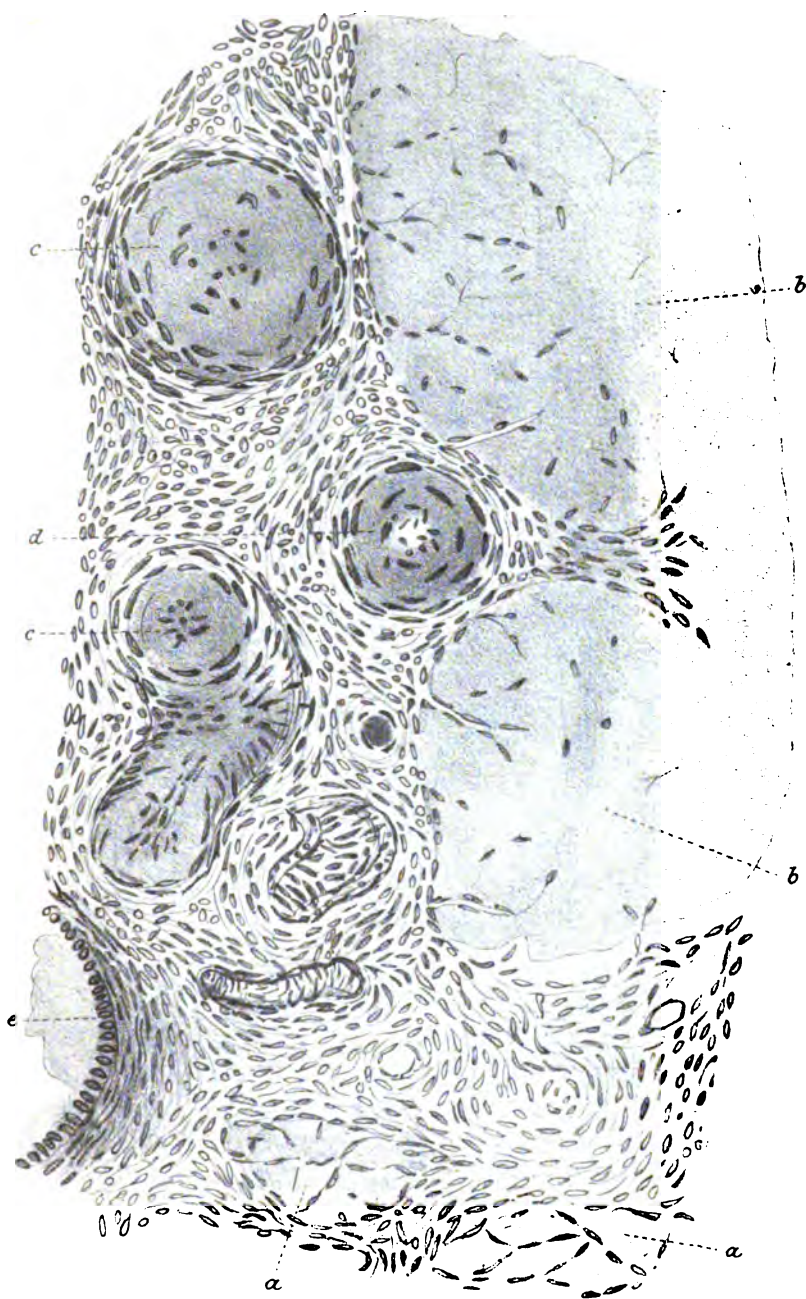


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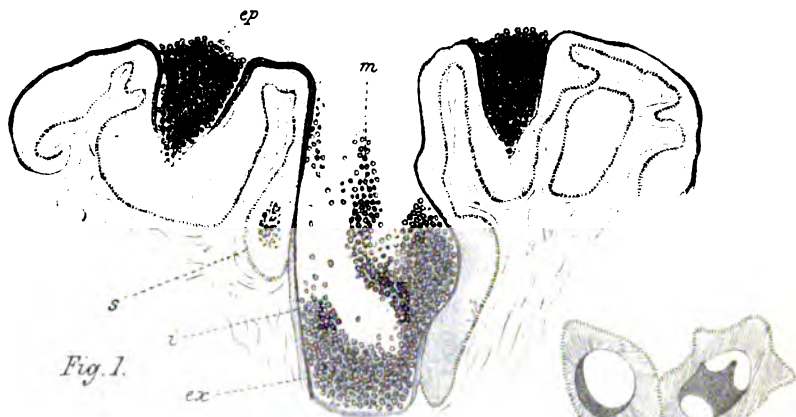
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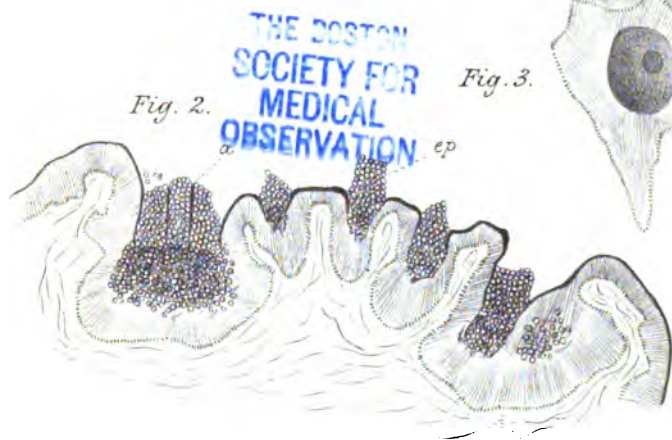








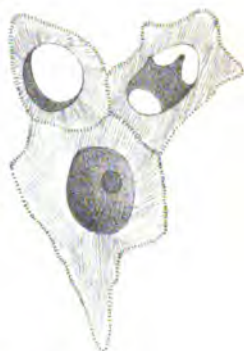
*Fig. 1.*



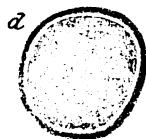
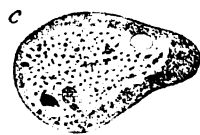
*Fig. 2.*

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*Fig. 3.*



*Fig. 4.*





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**Journal of Anatomy and Physiology.**

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THE SEGMENTAL VALUE OF THE CRANIAL NERVES.

By A. MILNES MARSHALL, M.A., D.Sc., *Fellow of St. John's College, Cambridge, Professor of Zoology in Owens College.*  
(PLATE X.)

WHETHER the nerves arising from the brain are directly comparable to those taking their origin from the spinal cord, and, if so, to how many pairs of the more symmetrically arranged spinal nerves the cranial ones are equivalent, are questions which have attracted the attention and exercised the ingenuity of many of the greatest anatomists, and ones which have been answered in the most varied senses by the different writers who have attempted to grapple with their difficulties. So long as the problems were attacked from the morphological side alone, as was the case with all the earlier attempts to solve them, the answers obtained were vague, inconclusive, and mutually contradictory; but since the clear light of embryology has been directed upon them the clouds of uncertainty have been to a very considerable extent dispersed, and there is now, especially amongst those who have most recently dealt with these questions, a very considerable and satisfactory agreement as to the main outlines of the answers to be given, although in many points of detail there is still much discrepancy between the several accounts.

The present paper is an attempt to set forth the actual position of these problems, and the leading phases through which they have passed in their gradual maturation. In preparing it I have made use of the investigations of others, so far as known to me, as well as of my own published in this *Journal* and elsewhere.<sup>1</sup>

<sup>1</sup> A list of the works consulted is given at the end of this paper.

**Historical Sketch.**—The older writers relied exclusively on anatomical evidence in dealing with the problems before us, and their determinations were rather of the nature of guesses than logical endeavours to grapple seriously with the difficulties encountered. Moreover, in the great majority of cases their judgment was influenced in a very prejudicial manner by pre-conceived ideas on the morphological constitution of the skull.

Inasmuch as these older theories are all based on the same arguments, and differ from one another only in points of minor importance, it will be sufficient to take one of them and examine it critically. For this purpose I select the theory advanced by Stieda, the most recent, indeed the only recent, advocate of the views in question.

Stieda,<sup>1</sup> in attempting to solve the problem of the segmental value of the cranial nerves, commences by stating that as he accepts Oken's theory that the skull consists of three vertebræ, the number of pairs of segmental cranial nerves must necessarily be two; viz., a pair leaving the skull between the first and second skull-vertebræ on either side, and a pair emerging between the second and third skull-vertebræ, the nerves passing out between the skull and the first cervical vertebra being universally considered, when present, the first pair of *spinal nerves*.

Having in this very summary manner determined the number of segmental cranial nerves, Stieda proceeds to divide the nerves actually present into two groups in accordance with this determination. He first rejects the nerves of special sensation, *i.e.*, the olfactory, optic, and auditory, on the ground that *embryology shows them to be really parts of the brain, and therefore not directly comparable with the other nerves*.

Concerning the remaining nine pairs of nerves still left for consideration, he holds that the most reliable evidence is afforded by the fact that *in certain groups of animals some of these nerves do not arise independently from the brain, but are represented by branches of other nerves*.

Reasoning from these data, Stieda comes to the conclusion that the component factors of his first cranial segmental nerve

<sup>1</sup> Stieda, *Studien über das centrale Nervensystem der Wirbelthiere*, Leipzig, 1870. *Separat-Abdruck aus der Zeitschrift für wissenschaftliche Zoologie*, Bd. *xx*. p. 166, *seq.*

are the third or oculomotor, the fourth or trochlear, the fifth or trigeminal, the sixth or abducent, and the seventh or facial nerves; and that of these the third, fourth, sixth, and seventh nerves, and the motor root of the fifth together represent the anterior or motor root, while the sensory portion of the fifth nerve is the representative of the posterior or sensory root. In support of these conclusions he adduces the following arguments:—

1. That the three eye-muscle nerves and the facial nerve may sometimes be replaced by branches of the trigeminal,<sup>1</sup> and therefore may be considered as belonging primarily to that nerve.

2. That the three eye-muscle nerves, the facial nerve, and the *portio minor* of the trigeminal behave with reference to their origin from the brain like the anterior roots of the spinal nerves; the *portio major* of the trigeminal, on the contrary, like a posterior root: meaning by this, the relations of the nerves in question to the nuclei of origin within the substance of the brain.

The second or posterior cranial segmental nerve he considers to be made up of the ninth or glosso-pharyngeal, the tenth or vagus, the anterior roots of the eleventh or spinal accessory, and the twelfth or hypoglossal nerves; the ninth, tenth, and anterior roots of the eleventh pair making up the posterior root, and the twelfth nerve representing the anterior or motor root, the main grounds of determination being the same as those relied on in the case of the supposed first nerve.

I have quoted Stieda at some length mainly in order to direct attention to the nature of the evidence on which he attempts to solve the question. The main points on which he relies are contained in the passages I have italicised above, viz., (1) *that the nerves of special sense are contrasted with the other cranial nerves as being, properly speaking, parts of the brain and not nerves in the strict sense of the word*; and (2) *that in certain groups of animals one or more of the cranial nerves may lose their more usual independent character, and appear as, or be replaced by, branches of some other nerve; and further, that this is to be taken as indicating that the nerves in question were originally branches of this other*

<sup>1</sup> All the cases in which this replacement is alleged to occur will be discussed later on in this paper.

*nerve, and that their independent origin from the brain, when it does occur, is a secondarily acquired feature.*

Now, these two points are of primary importance, forming, as is at once seen, the whole basis of Stieda's argument, and in relying on them he is very far from standing alone. Indeed, until some five or six years ago their correctness has been assumed, either tacitly or explicitly, by the great majority of those who have dealt with the question, including some of the most eminent anatomists of the time, such as J. Müller,<sup>1</sup> Arnold,<sup>2</sup> Langer,<sup>3</sup> Gegenbaur,<sup>4</sup> and, though in a somewhat less positive manner, Huxley.<sup>5</sup> I direct attention to this at once, because we shall find further on that there are very strong reasons for holding that neither of the points in question is really correct. I have taken Stieda as the most recent representative of a school to which C. V. Carus, Arnold, Buchner, J. Müller, Langer,<sup>6</sup> and many other prominent anatomists belonged, a school which attacked the problem of the segmental value of the cranial nerves by first determining perfectly independently the number of segments or skull-vertebræ in the head, a determination made as a rule on very insufficient and often purely fanciful grounds, and having thus decided the *number* of segments, and therefore of segmental nerves, proceeding to apportion the several nerves to these segments, usually in a very arbitrary manner. The writers named above differ, indeed, in the number of head-segments they respectively adopt, but agree in the principle on which they work, viz., *determining the number of segmental nerves from that of the supposed segments or vertebræ composing the skull.*

Stannius was the first to deal with the question in a more philosophical spirit, and to attempt to determine the number of segmental nerves by a direct study of the nerves themselves.

<sup>1</sup> Joh. Müller, *Handbuch der Physiologie des Menschen*, 1844, p. 631.

<sup>2</sup> Arnold, *Handbuch der Anatomie des Menschen*, 1851, Bd. ii. pp. 830-834.

<sup>3</sup> Langer, *Lehrbuch der Anatomie des Menschen*, 1865, p. 429.

<sup>4</sup> Gegenbaur, "Über die Kopfnerven von Hexanchus," *Jenaische Zeitschrift*, Bd. vi. 1871, pp. 548-551.

<sup>5</sup> Huxley, *The Anatomy of Vertebrated Animals*, 1871, pp. 71-74.

<sup>6</sup> An excellent summary of the views of these and other writers on the segmental value of the cranial nerves will be found in Stieda's paper already quoted. They all agree in principle with the account given above, the differences being merely in points of detail.

The results of his investigations<sup>1</sup> are contained in his invaluable treatise on the Peripheral Nervous System of Fishes published in 1849. He leaves the three nerves of special sense out of consideration for the same reason as Stieda and the other anatomists we have mentioned, *i.e.*, that they are rather parts of the brain than true nerves. He also omits the eye-muscle nerves, remarking that any attempt to homologise them with spinal nerves "encounters insuperable difficulties on account of their peculiar origin, their absence of ganglia, and their exclusive distribution to the muscles of a sensory apparatus, which are in no way comparable with the muscles of the vertebræ." The remaining nerves, however, Stannius deals with in a very complete and masterly manner. He considers that the fifth, seventh, ninth, and tenth nerves are each equivalent to a spinal nerve, and compares in detail both the roots of origin and the branches of these nerves with those of the spinal nerves.

Stannius was also the first to point out the very important relations of the ventral branches of these segmental cranial nerves to the visceral arches. In the essay quoted above he shows how each visceral arch is supplied by two branches belonging to different nerves, one running along its anterior border, and one along the posterior. He points out how the first branchial arch is supplied along its anterior border by the glosso-pharyngeal nerve, and along its posterior by the vagus; how the remaining branchial arches are supplied by the vagus, each arch by branches from separate stems; how the hyoid arch is supplied in front by the hyoidean branch of the facial nerve, and behind by the anterior branch of the glosso-pharyngeal; how the mandibular arch has the mandibular branch of the trigeminal nerve along its anterior border, and along its posterior the anterior branch of the facial, which he identifies as the *chorda tympani* of *Aves* and *Mammalia*; and finally, how the upper jaw is supplied by the ophthalmic and maxillary divisions of the fifth nerve.

He concludes this portion of his treatise with the following very suggestive sentence:—"Hence it follows that the number of the ventral branches of each cranial nerve, and the number of

<sup>1</sup> Stannius, *Das peripherische Nervensystem der Fische*, Rostock, 1849, pp. 125-181.

the spinal-like (segmental) cranial nerves is not determined so much by the number of the skull-vertebræ as by that of the visceral arches."<sup>1</sup>

In thus stating that the number of segmental cranial nerves was no longer to be determined by preconceived ideas concerning the composition of the skull, but by direct examination of the nerves themselves, Stannius rendered an invaluable service to morphology. Had he, indeed, gone one step further; had he been able to completely disabuse his mind of this notion of skull-vertebræ which was exercising so pernicious an influence on the zoologists of the day, he would have anticipated by more than twenty years Gegenbaur's announcement<sup>2</sup> of that theory of the vertebrate skull which has since, with some slight modifications, been accepted almost universally.

While the school of morphologists we first dealt with determined the number of the segmental nerves by that of the skull-segments, Stannius showed conclusively that there was no relation whatever between the two, but that there was a very definite and remarkable one between the segmental nerves and the visceral arches. Gegenbaur went a step further, and, starting with the segmental nerves and visceral arches, determined from them the number of head-segments, thus completely reversing the order of proceeding of the older school.

Gegenbaur is sometimes credited with being the first to establish the relations of the cranial nerves to the visceral arches, a determination which, as we have seen, had been already made by Stannius. The often quoted table of the cranial nerves given by Gegenbaur,<sup>3</sup> contains, in fact, nothing that had not been already pointed out by Stannius, except an attempt to rank the labial cartilages as visceral arches, an attempt which has not met with general acceptance. Gegenbaur's real merit consisted in pointing out that the ideal number of skull-vertebræ, as determined by Oken and other "transcendental anatomists," was to be left out of consideration altogether; that the evidence offered by the cranial nerves and visceral arches was to be accepted in

<sup>1</sup> Stannius, *op. cit.* p. 131.

<sup>2</sup> Gegenbaur, "Ueber die Kopfnerven von Hexanchus," *Jenaische Zeitschrift*, 1871.

<sup>3</sup> Gegenbaur, *loc. cit.* p. 552.



full, and was to be taken as the basis for determining the number of segments in the head; and that the vagus nerve was, from the fact of its supplying more than one visceral cleft, to be considered as equivalent to more than one segmental nerve, and to be regarded as formed by the fusion of a certain number of primitively distinct nerves.

Thus it has come to pass that the cranial nerves, while formerly considered of very subordinate importance, are now recognised as affording a very valuable and reliable clue to the solution of that favourite morphological problem—the segmentation of the vertebrate head; and Gegenbaur's paper, which was undoubtedly the chief means by which the cranial nerves were rescued from their former dependent position, must be viewed as marking a most important era in their history.

Attention being thus pointedly directed to the cranial nerves, their comparative anatomy and embryology quickly engaged the attention of zoologists; and during the last five or six years our knowledge on these points has received very material additions, additions which have, on the whole, tended to strongly confirm Gegenbaur's views, while causing modification of them in many secondary points.

The most important of these more recent contributions is undoubtedly the series of facts brought to light by Balfour concerning the early stages of development of the spinal and cranial nerves in elasmobranch fishes. Balfour showed that,<sup>1</sup> contrary to the generally accepted theory, the nerves are outgrowths from the cerebral nervous system, and therefore of epiblastic origin, instead of being, as formerly supposed, structures arising independently in the mesoblast and only acquiring a secondary connection with the brain and cord.

In the case of the spinal nerves, he showed that the two roots, anterior and posterior, arise separately and independently; that the posterior roots are local outgrowths of a continuous longitudinal band—the neural crest—which grows out along the mid-dorsal line of the spinal cord (fig. 1). By lateral growth of the dorsal summit of the cord the nerve roots of the two sides, which

<sup>1</sup> Balfour, "On the Development of the Spinal Nerves in Elasmobranch Fishes," *Phil. Trans.* vol. clxvi. pt. 1, 1875; and *A Monograph on the Development of Elasmobranch Fishes*, 1878, pp. 156-161 and 191-205.

are at first (fig. 1) directly continuous with one another across the top of the cord, become separated to a certain extent (fig. 2). The nerve root on either side grows downwards, closely applied to the side of the cord, it then acquires a new or secondary attachment<sup>1</sup> to the side of the cord, some little distance below the primary one (fig. 3). A little later the primary attachment disappears, and the secondary alone remains as the permanent attachment of the posterior root to the cord (fig. 4).

The anterior roots arise later than the posterior, each as an independent conical outgrowth from the latero-ventral angle of the cord (fig. 3). The roots grow rapidly, and soon form elongated bands of fusiform cells, which retain their original points of origin from the cord. Each is at first, and for some time, quite distinct from the posterior root (fig. 3), with which, however, it subsequently unites to form the adult spinal nerve (fig. 4).

Further differences between the anterior and posterior roots are afforded by the fact that the posterior develops at a very early period a large ganglionic swelling—the future spinal ganglion—(figs. 3 and 4), while the anterior root is devoid of ganglion cells. The roots of origin of the anterior root are also very generally multiple, while those of the posterior roots, whether primary or secondary, are apparently invariably single.

Balfour's observations were soon extended to birds and mammals, and the description given above is now recognised as that of the general and typical mode of development of the vertebrate spinal nerves. It was further found that the neural crest is not confined to the spinal cord, but extends forwards along the top of the brain, and that certain of the cranial nerves are developed from it in the same way as the posterior roots of the spinal nerves. By this discovery a new and very reliable clue to the segmental value of the cranial nerves is obtained, for it is clear that if certain of the cranial nerves do, and others do not, conform to the mode of development of the typically segmental spinal nerves, there is strong reason for regarding the former as being of segmental value, and the latter as not.

Embryology has furnished us with one further test of the

<sup>1</sup> The account of this shifting is based on my own observations. Balfour expresses himself as "inclined to adopt this view" (*Comparative Embryology*, vol. ii. p. 372), but does not definitely do so.

segmental value of cranial nerves, for which again we are indebted to Mr. Balfour, who has shown that in elasmobranchs (and the observation has since been extended to other groups) the two halves of the coelom or body-cavity at an early period extend forward on either side of the neck into the head, and that on the appearance of the visceral clefts each of these halves becomes cut up into a series of isolated compartments, one in each visceral arch.<sup>1</sup> If the visceral clefts and arches are segmental, it is clear that these "head-cavities," as they are called, must be so also, and that they will therefore afford an additional clue to determining the segmental value of the nerves associated with them.

**Summary of Evidence of Segmental Value of Cranial Nerves.**—From what has been said above it will be evident that we have now several independent tests of the metameric or segmental value of the cranial nerves,—tests with *all* of which a nerve ought to comply to entitle it to rank as segmental. For convenience of reference, these tests, the majority of which have already been discussed, may be enumerated here:<sup>2</sup>—

1. Segmental nerves develop at a very early stage as outgrowths from the neural ridge on the dorsal surface of the brain.

2. At an early period they shift downwards, and acquire new or secondary roots of attachment to the sides of the brain.

3. The general course of the main stem of a segmental nerve is at right angles, or nearly so, to the axis of the head at the point of origin of the nerve. This feature, which is explained more fully in the paper quoted above, is evident from an inspection of fig. 8, in which the directions of the segmental nerves are shown, and from the consideration that the course of segmental nerves must be approximately parallel to the boundaries of the segments to which they belong: a segmental nerve could not run transversely across a number of segments.

4. Segmental nerves have the characteristic relations to the visceral clefts and arches, and, therefore, also to the head-cavities in these arches, first pointed out by Stannius as noticed above,

<sup>1</sup> Balfour, *Elasmobranch Fishes*, pp. 206–209; also Marshall, "Head-Cavities and Associated Nerves of Elasmobranchs," *Quart. Journ. of Micros. Science*, January 1881.

<sup>2</sup> Cf. Marshall, "The Morphology of the Vertebrate Olfactory Organ," *Quart. Journ. of Micros. Science*, July 1879, p. 317.

each nerve supplying the borders of one cleft, and therefore of two arches. Concerning this test, it may be noted that, although from the constancy of the relations of the visceral clefts to other structures in all vertebrates above *Amphioxus*, there can be no doubt that Gegenbaur, Huxley, Semper, and others are correct in maintaining the segmental value of these clefts, yet that the total absence of any correspondence between the visceral clefts and the body segments in *Amphioxus*, and still more in the Ascidians, makes it very doubtful whether this segmental character is a primitive one.

5. Segmental nerves very constantly present ganglionic enlargements, either at or near their points of division into their two main ventral branches.

Having thus cleared the ground, and explained what we mean by a segmental nerve, and why it is of importance to determine which of the cranial nerves are of segmental value, and which are not, I propose to consider these nerves and discuss their claims in order, beginning with the most anterior ones, and taking them in the sequence usually adopted by anatomists.

I. The First or Olfactory Nerve.—This nerve was until recently supposed, by reason of its development, to stand quite apart from the rest of the cranial nerves, and to be, properly speaking, a part of the brain rather than a nerve in the strict sense of the word.<sup>1</sup> Instead of developing like the other nerves, the olfactory was stated to arise as a hollow outgrowth from the anterior part of the cerebral hemisphere—the so-called olfactory lobe or vesicle: it was also stated to arise considerably later than the posterior cranial nerves.

It is now known that these supposed distinctions between the olfactory and the other nerves do not really obtain,<sup>2</sup> but, on the contrary, that the olfactory nerves develop in precisely the same way as the other cranial nerves; that they arise at first from the upper part of the fore-brain and gradually shift downwards, acquiring by so doing a secondary connection with the

<sup>1</sup> *Vide*, e.g., Huxley, *Anatomy of Vertebrate Animals*, p. 71; and Gegenbaur, *Elements of Comparative Anatomy*, English Translation, p. 515.

<sup>2</sup> Marshall, "Morphology of Vertebrate Olfactory Organ," *Quart. Journ. of Micros. Science*, July 1879; and Balfour, *Comparative Embryology*, vol. ii. 1881, pp. 336 and 382.

cerebral hemispheres, of which they are at first completely independent; and, finally, that the olfactory lobe or vesicle so far from being the earliest part to be developed is actually the last, no vestige of it appearing in the chick until the seventh day of incubation, in the salmon till long after hatching, or in dogfish until stage O of Balfour's nomenclature.

If, then, the olfactory nerve agrees in all important features of its development with the other cranial, and the spinal nerves, the further question at once suggests itself,—has it segmental value?<sup>1</sup> An examination of the evidence at our disposal, which is unfortunately far from complete, shows that there is much to be said in favour of such a view; thus, applying to the olfactory nerve the several tests of the metameric value of cranial nerves in the order given above, on p. 313, we obtain the following results:—

1. The olfactory nerve develops very early: the actual date of its first appearance is very difficult to determine, and has not yet been ascertained with certainty in any case, but in both the chick and the dogfish it appears at a very early stage of development, and in the chick, indeed, an attempt has been made to show that the olfactory nerve is “one of the first nerves in the body to appear,”<sup>2</sup> arising before any of the spinal nerves. There is also evidence, though as yet inconclusive, in favour of the origin of the olfactory nerve in the chick from the neural crest.

2. The olfactory nerve resembles the segmental nerves in undergoing during the earlier stages of its development a very considerable displacement of its root of attachment to the brain, and as this feature is one of the most remarkable characters of these segmental nerves, and is, so far as we know, confined to them, its occurrence in the olfactory nerve must be admitted to be of much weight.

In both the dogfish and chick the olfactory nerves are clearly recognisable before the cerebral hemispheres have commenced to develop, the nerves at this stage arising from the dorsal part of the sides of the original fore-brain or anterior cerebral vesicle.

<sup>1</sup> I have dealt with this question at some length in a former paper on “The Morphology of the Vertebrate Olfactory Organ,” *Quart. Journ. of Micros. Science*, July 1879, to which I would beg to refer the reader who may desire further details than I can give here.

<sup>2</sup> Marshall, “The Development of the Cranial Nerves in the Chick,” *Quart. Journ. of Micros. Science*, Jan. 1878, p. 23.

The hemispheres in the chick are lateral outgrowths of the fore-brain, and are from the first situated above, *i.e.*, on the dorsal side of the roots of the olfactory nerves; they grow forwards and upwards with great rapidity, driving the olfactory nerves down to the base of the brain, and so causing these nerves to appear to arise from their under and anterior part. Whether the root of the olfactory nerve undergoes any change comparable to the secondary attachment described above as occurring in the spinal nerves, has, however, not yet been ascertained.

3. The general course of the olfactory nerve in the early stages of development is, like the segmental nerves, at right angles to the axis of the head at the point of origin of the nerve, although, owing to cranial flexure, it is very far from being parallel to the hinder segmental nerves. This feature is shown in fig. 8, I. In the later stages of development, owing to the forward growth of the nasal region, this relation becomes completely lost.

4. Concerning the relations of the olfactory nerve to visceral arches and clefts, I must beg to refer the reader to the paper quoted above, in which I have drawn attention to "the very close resemblance as to form, structure, general relations, time of appearance, &c., existing between the olfactory organ and the gill clefts," and have adduced other arguments on which I have attempted to establish the following conclusions:—"That the olfactory organ is the most anterior visceral cleft; that the olfactory nerve is the segmental nerve supplying the two sides of that cleft in a manner precisely similar to that in which the hinder clefts are supplied by their respective nerves; and that the Schneiderian folds are homologues of gills."<sup>1</sup>

5. The olfactory nerve is distinctly ganglionic near its root of origin from the brain in elasmobranchs and in the chick.

It would thus appear that although the evidence is at present far from conclusive, and although further information is needed on many points, notably concerning the earliest stages of development of the olfactory nerve, yet that the nerve answers fairly well to the tests of segmental value as defined above; and it is important to note that the points in which it responds incompletely are precisely those on which our knowledge of the nerve

<sup>1</sup> "Morphology of Vertebrate Olfactory Organ," *Quart. Journ. of Micros. Science*, July 1879, p. 330.

is avowedly imperfect, and that in no case is a test directly contradicted. I am therefore disposed, while fully admitting the need for further investigation, to rank the olfactory nerve as the most anterior of the cranial segmental nerves, the nerve belonging to the first head-segment.

The segmental value of the olfactory nerve has recently been advocated by Wiedersheim, who draws attention to the fact that in *Epicrion*, and probably in other *Gymnophiona* as well, there are on either side two olfactory nerves, one dorsal and one ventral, the roots of the two being perfectly independent, and some little distance apart.<sup>1</sup> Wiedersheim considers that these two roots are homologues of the dorsal and ventral roots of a spinal nerve, and that by their discovery the segmental rank of the olfactory nerve may be considered to be established.

A similar condition of the olfactory nerve in *Pipa dorsigera* has been figured, though not described, by Fischer.<sup>2</sup>

These two cases, in both of which the additional root is the dorsal one, tend strongly to confirm the view taken above of the primitive connection of the olfactory nerve with the dorsal surface of the brain, and therefore presumably with the neural crest; but in the absence of any observations on either the development or the physiological properties of the two roots in question, I do not think that much weight can be attached to Wiedersheim's suggestion of their homology with the roots of a spinal nerve.

Balfour<sup>3</sup> argues against the segmental value of the olfactory nerve, on the ground that it is incompatible with the views which he holds concerning the primitive vertebrate mouth, and concerning the relations between the nervous systems of vertebrates and invertebrates. His views on these points are of very great interest and importance; but inasmuch as they involve the descent of Chætopods and Vertebrates, not from a common segmented ancestral type, but from a common unsegmented type, and also the existence of a group of segmented animals, which "appears now to have perished" without leaving any trace behind, it would clearly be impossible to discuss them

<sup>1</sup> Wiedersheim, *Die Anatomie der Gymnophionen*, Jena, 1879, pp. 59, 60, and pl. iv. fig. 35, pl. vi. fig. 62.

<sup>2</sup> Fischer, *Amphibiorum nudorum neurologiæ specimen primum*, 1843, Tab. ii. fig. 1.

<sup>3</sup> Balfour, *Comparative Embryology*, vol. ii. 1881, pp. 260-265 and 383.

here in full. His theory that the vertebrate fore-brain is the homologue of the supra-oesophageal ganglia of Arthropods and Chætopods is, however, to my mind open to very serious objections, some of the more weighty of which he has himself mentioned, viz. (1) that there is no actual anatomical or embryological break between the fore-brain and the hinder portion of the central nervous system, such as one might reasonably expect to find on his hypothesis; (2) that the lowest known vertebrate, *Amphioxus*, instead of lending any support to this view, distinctly contradicts it, the fore-brain being less differentiated from the hinder portion than in any other vertebrate, while "the termination of the notochord immediately behind the fore-brain"—almost the only direct evidence he adduces in favour of the "morphological distinctness" of the fore-brain—again fails completely, the notochord in *Amphioxus*, as is well known, extending to the extreme anterior end of the head, some distance beyond the front end of the brain.

**II. The Second or Optic Nerve.**—Although, as we have just seen, the statement that the olfactory nerve is rather a part of the brain than a nerve in the strict sense of the word is found on examination not to hold good, yet, as regards the optic nerve, it is certainly correct; the mode of development of the optic nerve, which is too well known to require a detailed description here, placing it in this respect in marked contrast to every other nerve in the body.

From the fore-brain or anterior cerebral vesicle two hollow lateral outgrowths arise—the optic vesicles. These become constricted at their origin from the brain, the constricted portions or optic stalks becoming ultimately the optic nerves. By a process of unequal growth of the different parts, coupled with a direct pushing in of the outer wall by the formation of the lens, each vesicle becomes doubled up on itself, the outer wall being pushed back into the inner, and so giving rise to the double-walled "optic cup" or secondary optic vesicle.

This mode of development, which, with secondary modifications applies to all vertebrates except *Amphioxus*, and must therefore be considered as primitive so far as vertebrates are concerned, differs so fundamentally from the development of the



hinder cranial or spinal nerves that no comparison whatever is possible between them. The optic nerve must therefore be regarded as one *sui generis*, and as one which can accordingly have no claim to be considered of segmental value.

The existence of this clearly non-segmental nerve between the olfactory and the hinder nerves is undoubtedly an objection to the view advocated above concerning the segmental value of the olfactory nerve; but until we obtain a clearer light than we are at present able to throw on the phylogenetic history of the vertebrate eye, and indeed of the vertebrate race altogether, it is difficult to gauge properly the weight of the objection.

**The Eye-Muscle Nerves.**—Concerning the morphological value of these three nerves—the third, fourth, and sixth pairs—opinions have perhaps differed more than in the case of any of the other cranial nerves.

The nerves in question are small, with a singularly limited and constant distribution to the muscles moving the eyeball, and to certain other parts in connection with the eye, the third nerve supplying the *rectus superior*, *rectus internus*, *rectus inferior*, and *obliquus inferior* muscles of the eyeball, also the *levator palpebræ superioris* and the circular muscle of the iris; the fourth nerve supplying the *obliquus superior* muscle, and in many vertebrates sending sensory branches to the conjunctiva and the skin of the upper eyelid; and the sixth nerve supplying the *rectus externus* muscle, and in many forms the suspensory muscle of the bulb of the eye and the muscles of the nictitating membrane. In dealing with them it will be convenient to consider them at first collectively, inasmuch as many points of importance concern them all alike, and afterwards to consider briefly the several points of individual interest which they present respectively.

Until very recently it was the almost universal custom amongst anatomists, when discussing the segmental value of the cranial nerves, to exclude the eye-muscle nerves altogether from consideration, on the ground that *they were not constant in their distribution, but that one or more of the muscles normally supplied by them might under special circumstances be supplied by branches of the fifth nerve*, the further inference being drawn from these

special cases that the eye-muscle nerves were primitively branches of the fifth nerve, which have in the majority of existing vertebrates attained independence and acquired the appearance of distinct nerves, a title to which they have really no claim.<sup>1</sup>

This view has very recently indeed been advocated by Wiedersheim, whom I quote in order to illustrate my statements. In dealing with the fourth nerve in the frog he notices that it usually forms anastomotic communications with the ophthalmic branch of the fifth nerve as it crosses it, and that the number of these communicating branches is very variable. He then says:—"Dies eben beschriebene Verhalten sowie auch dasjenige des *Abducens* und des später abzuhandelnden *Oculomotorius* liefert eine hübsche Illustration zu der in höheren Thiergruppen in immer stärkerer Weise hervortretenden Tendenz der Augenmuskelnerven, sich von ihrem Stammboden, der Trigeminiisgruppe, zu emancipiren, um endlich eine gut individualisirte Selbstständigkeit zu erlangen."<sup>2</sup>

As this view, so definitely expressed by Wiedersheim in the above passage, appears to have met with very general acceptance, and as it very seriously affects and concerns the subject of the present paper, I have taken some trouble to collect all the recorded cases in which the distribution of the eye-muscle nerves or the supply of the eye-muscles in vertebrates is said to present any constant deviation from the normal arrangement as noticed above; and I propose now to examine critically these alleged exceptions to the general rule.

1. *Amphioxus*.<sup>3</sup>—The azygos character of the eye and its extreme simplicity of structure render any comparison with the eyes of higher and more typical vertebrates perfectly futile.

2. *Marsipobranchii*.

(a) *Hyperotræti*.—Among the myxinoid fishes, according to

<sup>1</sup> Vide, e.g., Gegenbaur, "Ueber die Kopfnerven von *Hexanchus*," *Jenaische Zeitschrift*, 1871, pp. 548, 549; Huxley, *Anatomy of Vertebrated Animals*, 1871, p. 73; also Stieda and the various authors quoted by him in his "Studien ueber das centrale Nervensystem der Wirbelthiere," *Zeitschrift für wissenschaftliche Zoologie*, Bd. xx. 1870.

<sup>2</sup> Wiedersheim, in Ecker's *Anatomie des Frosches*, Zweite Abtheilung, 1881, p. 24, note 1.

<sup>3</sup> Stannius, in his *Handbuch der Anatomie der Wirbelthiere*, Zweite Auflage, 1854, p. 161, notices the absence of the eye-muscle nerves in *Amphioxus*.

Stannius,<sup>1</sup> J. Müller,<sup>2</sup> Huxley,<sup>3</sup> Gegenbaur,<sup>4</sup> and others, the eye-muscle nerves are completely absent. Here again we are dealing with animals in which the eyes are in a very rudimentary condition, and the eye-muscles either absent or extremely imperfectly developed; so that, as pointed out by Schwalbe,<sup>5</sup> no importance can be attached to them in determining the question of the primitive independence of the eye-muscle nerves, and this consideration is much strengthened by the strong evidence we possess of the Myxinoids being degenerate or degraded forms.<sup>6</sup>

(b) *Hyperoartii*.—Attention has been directed to the condition of the eye-muscle nerves in the lampreys by a number of writers. According to Schlemm and d'Alton,<sup>7</sup> the lampreys have independent eye-muscle nerves, but their number is diminished, and some of the muscles are supplied by the fifth nerve. The fourth nerve is described as having its usual origin behind the optic lobes and entering the orbit in company with the third, which has an independent origin in front of that of the fifth. The combined nerve, formed by the union of the third and fourth, divides into two main branches, an *upper one* supplying the *rectus superior*, and a *lower one* supplying the *rectus internus* and *obliquus superior*. The three other muscles, viz., *rectus inferior*, *rectus externus*, and *obliquus inferior*, are said to receive their nerves from the trunk of the fifth nerve.

Fischer<sup>8</sup> and Stieda<sup>9</sup> also refer to the peculiar distribution of the eye-muscle nerves in the lampreys, but avowedly draw their information from Schlemm and D'Alton's paper, from which it would appear that Huxley,<sup>10</sup> and probably Owen<sup>11</sup> and Günther<sup>12</sup> also, derive their accounts.

<sup>1</sup> Stannius, *op. cit.* p. 161.

<sup>2</sup> J. Müller, *Vergleichende Neurologie der Myxinoiden*, p. 49.

<sup>3</sup> Huxley, *Vertebrates*, p. 73.

<sup>4</sup> Gegenbaur, *Hexanchus*, p. 549.

<sup>5</sup> Schwalbe, "Das Ganglion Oculomotorii," *Jenaische Zeitschrift*, Bd. xiii. p. 71.

<sup>6</sup> Cf. Balfour, *Comparative Embryology*, vol. ii. 1881, p. 263, note 2.

<sup>7</sup> Schlemm u. D'Alton, "Ueber das Nervensystem der Petromyzon," *Müller's Archiv*, 1838.

<sup>8</sup> Fischer, *Amphibiorum nudorum Neurologia Specimen Primum*, 1843, p. 47.

<sup>9</sup> Stieda, *loc. cit.* p. 174.

<sup>10</sup> Huxley, *Anatomy of Vertebrated Animals*, p. 73.

<sup>11</sup> Owen, *Anatomy of Vertebrates*, 1866, vol. i. p. 300.

<sup>12</sup> Günther, *Introduction to the Study of Fishes*, 1860, p. 105.

Gegenbaur<sup>1</sup> gives a slightly different account. He says that in *Petromyzon* there is an independent fourth nerve, but that the sixth is a branch of the fifth, which supplies the *rectus inferior* as well as the *rectus externus*, while the third nerve is limited in its distribution, supplying the *rectus superior*, *rectus internus*, and *obliquus inferior*. He gives no reference in support of his statement, and must therefore be supposed to make it on his own authority, especially as it differs notably from the accounts of all other writers whom I have been able to consult.

Concerning the above accounts, it appears that they can be reduced to two sources—(1) the description given in 1838 by Schlemm and D'Alton, which I have assumed to be the source from which Owen, Huxley, and Günther obtain the accounts given in their text-books quoted above, because their descriptions, which are very brief, agree exactly with that of Schlemm and D'Alton, and add nothing to it; and (2) Gegenbaur's description in 1871, which must be independent, inasmuch as it does not quite agree with Schlemm and D'Alton's. According to Gegenbaur, the only peculiarity is that the sixth nerve is not independent but a branch of the fifth, which supplies the *rectus inferior* as well as the *rectus externus*; while, according to Schlemm and D'Alton, three of the muscles—the *rectus inferior*, *rectus externus*, and *obliquus inferior*—are supplied by the fifth nerve; and, in addition to this, the third and fourth nerves unite together, a point which Gegenbaur does not notice.

The dissection is a difficult one, on account of the small size of the nerves concerned; and additional evidence from direct observation is necessary before we can decide whether either of the above descriptions is perfectly correct.

There are, however, certain points of considerable importance which concern not only *Petromyzon*, but many other forms as well, and may be conveniently dealt with here.

Both the third and fourth nerves are distinctly stated to have independent roots of origin, and to arise from the normal situations in the brain; and this being the case, I wish to point out that the anatomical arrangement of the nerves would probably be more correctly described by saying that the third nerve, though having a separate root of origin, becomes connected

<sup>1</sup> Gegenbaur, *Hexanchus*, p. 549, note 1.

with the fifth, so that in the adult some of its branches appear to be derived from the fifth; than by saying, with Huxley Stieda, and Günther, that the muscles in question are supplied by branches of the fifth.

Fischer long ago adopted this view. He describes, on Schlemm and D'Alton's authority, the condition of the nerves in *Petromyzon* in these words:—"Genus *Petromyzon* duas Oculomotorii ostendit partes, alteram liberam, parisque quarti quoque continentem fibras, alteram cum Trigemino conjunctam;"<sup>1</sup> and Stannius gives still clearer expression to it; for after referring to Schlemm and D'Alton's observations, he says:—"Offenbar ist hier ein Theil der Wurzelemente des N. oculorum motorius, so wie auch die Wurzel des N. abducens, in die Bahn des N. trigeminus, übergetreten,"<sup>2</sup> and remarks that it is quite possible that a very careful examination of the nerve-roots would show that the *abducens* has really an independent root of origin.

The point at issue is an important one, and must be clearly stated. When we find two nerves—the third and fifth, which in the great majority of vertebrates are independent of one another both in origin and distribution—in certain forms, as the lampreys, arising from the brain independently and normally, but becoming united together at some point or other of their course, so that it is no longer possible from mere anatomical observation to say with certainty to which of the two a given branch belongs, are we to infer, as is done tacitly or explicitly by many writers,<sup>3</sup> that the condition shown by the lamprey is the more primitive one, and represents an intermediate stage in the process by which the eye-muscle nerves gradually emancipated themselves from their parent nerve—the fifth—and attained ultimately the complete independence they show in the great majority of existing vertebrates? Or, on the other hand, are we to infer that the independent origin of the third nerve is primitive, and that its connection with the fifth, when, as in the lamprey, it does occur, is a secondarily acquired one? To my mind there can be no doubt whatever that the latter is the

<sup>1</sup> Fischer, *op. cit.* p. 47, note 1.

<sup>2</sup> Stannius, *Das periphere Nervensystem der Fische*, p. 18.

<sup>3</sup> Cf. the authors mentioned above, and especially the passage quoted from Wiedersheim on p. 320 above.

correct explanation ; and the chief reasons that lead me to think so are the following :—

(a) Though we know of instances—notably in the case of the vagus—of nerves originally distinct and independent gradually becoming fused, and then this fusion getting thrown back to a very early developmental stage ; yet we know of no established case of a branch attaining independence, and acquiring the character of a distinct nerve.

(b) Supposing it were possible for such a process to occur, it would certainly be very surprising if, as in the supposed case of the third nerve, the process of differentiation should commence at the proximal end, and that there should be a stage in which the roots were independent and the two nerves still fused distally.

(c) There are very strong reasons, which we shall discuss later on, for viewing both the third and fifth nerves as segmental, and therefore primitively independent of one another.

(d) If Wiedersheim's view were correct, we should certainly expect the third nerve of higher vertebrates in its early stages of development to show some indication of its supposed primitive connection with the fifth. So far, however, is this from being the case, that in all cases where the development of the third nerve has yet been traced, it is a perfectly independent nerve from the start.<sup>1</sup>

(e) A crucial test is afforded by the fact that other nerves—*e.g.* the fifth and seventh—though, as a rule, separate from one another throughout the vertebrate series, may in some forms become more or less closely united together, so that it is impossible by mere anatomical evidence to distinguish branches of the one from those of the other ; the forms in which this fusion of the fifth and seventh nerves occurs being, as we shall see more fully later on, in many cases the same as those in which the fifth and the eye-muscle nerves tend to fuse. In the case of the dog-fish, in which this fusion of the fifth and seventh nerves is a marked feature of the adult state, all the stages of develop-

<sup>1</sup> Marshall, "On the Development of the Cranial Nerves in the Chick," *Quart. Journ. of Micros. Science*, Jan. 1878, pp. 23-27 ; and "On the Head Cavities and Associated Nerves of Elasmobranchs," *Quart. Journ. of Micros. Science*, Jan. 1881, pp. 78-83.

ment are now known,<sup>1</sup> and it is found that, so far from the state of fusion being a primitive one, the two nerves are in their early stages quite independent and some distance apart, as in other vertebrates, and that their subsequent gradual approximation and fusion are purely secondary characters.

The above arguments appear to me to establish the proposition that the third nerve is primitively an independent one,<sup>2</sup> and that its partial fusion with the fifth, when it occurs, is a purely secondary and not a primary character.

If they prove the case for the third nerve, so also for the fourth and sixth nerves. The presence of independent roots of origin from the brain must be held to establish that, however close may be the connection of their trunks with the fifth nerve, they are really independent nerves, and are *not* to be described as being "replaced by branches of the fifth nerve."

In the case of the lampreys, then, I hold that we have no reliable evidence of the third or fourth nerves being in any way abnormal in their distribution to the eye-muscles; while, as regards the sixth nerve, although no distinct root of origin has yet been seen, I hold, with Stannius, that a much more careful and searching investigation must be made for it before any statement as to its absence can be accepted.

3. *Ganoidæi*.—In the majority of ganoids the nerves of the eye-muscles have the normal arrangement, and are completely independent of the fifth, except where the third unites with the ophthalmic branch of the fifth at the ciliary ganglion. Only one exception is known.

In *Lepidosteus*, according to J. Müller,<sup>3</sup> the arrangement is abnormal, the third and fourth nerves entering the orbit closely united with the ophthalmic division of the fifth, of which they appear as branches. The sixth nerve is described and figured as accompanying the main trunk of the fifth, but distinct from it.

<sup>1</sup> Marshall and Spencer, "Observations on the Cranial Nerves of Scyllium," *Quart. Journ. of Micros. Science*, July 1881, pp. 482-486.

<sup>2</sup> The independence of the third nerve has recently been upheld on anatomical grounds by Schwalbe—*Das Ganglion Oculomotorii*; and by Balfour, on embryological ones—*Comparative Embryology*, vol. ii.

<sup>3</sup> J. Müller, *Ueber den Bau und die Grenzen der Ganoiden*, 1846, p. 97, and plate iv. figs. 2 and 3.

Stieda, in his essay<sup>1</sup> before referred to, quotes Müller's account, but does so incorrectly, making Müller say that there is a distinct fourth nerve, but that the third and sixth are replaced by branches of the fifth; whereas Müller really says that the sixth is a distinct nerve, and that the third and fourth are not "replaced by branches of the fifth," but contained in the ophthalmic nerve.

Stannius,<sup>2</sup> referring to Müller's account, observes that it is probably merely another instance of juxtaposition of originally distinct nerves.

Concerning this alleged exception, we notice in the first case that it rests on a solitary description, which has not yet been confirmed, and that confirmation is needed is evident from the figures referred to. Müller gives two figures of the cranial nerves of *Lepidosteus*, which do not agree in all points with another; indeed, the points of difference are so marked that the two figures are by no means easy to reconcile with one another. Müller's figure 3 appears to me to present nothing exceptional, except that the third and fourth nerves enter the orbit as one trunk, and that the fourth nerve at the point where it crosses the *portio minor* of the *ramus ophthalmicus superficialis*<sup>3</sup> is rather more extensively connected with this nerve than is usually the case. The nerve which Müller marks  $\gamma$ , and calls the "ophthalmic branch of the fifth," but which he does not seem to have followed to the brain, I see no reason for considering as other than what one would naturally suppose it to be from its distribution to all the eye-muscles except the *rectus externus*, i.e., the combined third and fourth nerves. In Müller's figure 2 there is a remarkable point of difference, inasmuch as the nerve which I have considered in the former figure to be the proximal part of the *portio minor*, or trigeminal portion of the *ramus ophthalmicus superficialis*, is *entirely omitted*. No mention is made either in the text or in the description of the figures of this very important difference. I would further notice that, although the two figures in question are drawn of the same size

<sup>1</sup> Stieda, *loc. cit.* p. 174.

<sup>2</sup> Stannius, *Das periphere Nervensystem der Fische*, p. 19.

<sup>3</sup> For the nomenclature of these ophthalmic nerves, *vide* Marshall and Spencer, "Observations on the Cranial Nerves of Scyllium," part i., *Quart. Journ. of Micros. Science*, July 1881.



and to the same scale, yet that the relative proportions of the several nerves, and more especially the extent to which they are fused with one another, are so very different in the two cases that one is driven to suppose either that the figures are taken from different specimens, in which case there must be considerable individual variability in the very points alleged to be exceptional, or else that one or other of the figures is taken from an incomplete dissection.

The above considerations lead to the conclusion that, in the absence of direct confirmation, Müller's account of the eye-muscle nerves in *Lepidosteus* does not prove that they are in any way exceptional, except in the fact of the third and fourth nerves entering the orbit as one trunk.

Very important information concerning these nerves in *Lepidosteus* has recently been afforded by Schwalbe, who finds, from a careful examination of the nerves and brain, that *both the third and fourth nerves have independent origins from the brain*; <sup>1</sup> a fact which, as in the case of *Petromyzon*, must be held to conclusively prove that such connection as may actually occur between the fifth nerve on the one hand, and the third and fourth on the other, beyond their roots of origin, is of a purely secondary character, and that it does not in the very slightest degree militate against the claims of the third and fourth to rank as independent cranial nerves.

4. *Teleostei*.—The only recorded instances that I can find of deviation from the normal arrangement of the eye-muscle nerves among osseous fish are :—

(a) *Amblyopsis*,<sup>2</sup> the blind fish of the Mammoth cave of Kentucky, in which the eyes are rudimentary and functionless, and the eye-muscle nerves, as might be expected, absent.

(b) *Silurus glanis*, in which, according to Stannius,<sup>3</sup> the eyes are small, the eye-muscles very slender, and the eye-muscle nerves *outside the skull* closely united with the ophthalmic branch of the fifth. Stannius points out, however, that careful examination shows that all three nerves arise independently from the

<sup>1</sup> Schwalbe, *Das Ganglion Oculomotorii*, pp. 28, 72, and 73.

<sup>2</sup> Noticed by Stannius, *Das peripherische Nervensystem*, p. 18; and Schwalbe, *loc. cit.* p. 71.

<sup>3</sup> Stannius, *op. cit.* pp. 18, 19.

brain at the normal situations, and expressly notices that, but for the discovery of these extremely slender roots, the eye-muscle nerves of *Silurus* would have been beyond all doubt described as branches of the fifth nerve. It is of course probable that in the other species of blind fish, whether living in caves, as *Typhlichthys*, *Stygicola*, *Gronias*, *Ailia*, &c., or living at great ocean depths, as the *Scopelidæ*, the eye-muscle nerves are, as in *Amblyopsis spelæus*, rudimentary or absent; but it will be sufficiently evident, from what has been already said, that neither these blind fish nor such cases as *Silurus* tell in any way against the independent rank of the eye-muscle nerves.

5. *Dipnoi*.—In his account of the African *Lepidosiren* (*Protopterus*) *annectens*, Prof. Owen<sup>1</sup> notices that the optic nerves "are remarkably small, in correspondence with the feebly-developed organs of vision;" also that the eyeball "has no special muscles, whence the absence of the third, fourth, and fifth cerebral nerves."

According to Hyrtl<sup>2</sup> in the South American form, *Lepidosiren paradoxa*, in which also the eyes are very small, the four *recti* muscles are present, but the two *obliqui* not represented. The eye-muscle nerves were not found, but were believed to be replaced by two fine branches of the ophthalmic division of the fifth nerve, which branches, however, were not traced into the *recti* muscles.

Prof. Humphry's<sup>3</sup> description of *Lepidosiren* (*Protopterus*) *annectens* very closely agrees with Hyrtl's of *L. paradoxa* in the points with which we are now concerned. He finds, contrary to Owen, that the four *recti* muscles "may clearly be distinguished," though there are no *obliqui*. "Special nerves to these muscles (the third, fourth, and sixth) were not found;" but the ophthalmic division of the fifth is described as giving off in the orbit "ciliary and oculo-motor nerves," which, however, do not appear to have been traced to their distribution.

Gegenbaur<sup>4</sup> simply quotes Hyrtl to the effect that all three

<sup>1</sup> Owen, "Description of the *Lepidosiren annectens*," *Trans. Linnæan Soc.*, vol. xviii. 1839, p. 340.

<sup>2</sup> Hyrtl, "*Lepidosiren paradoxa*," Prag. 1846, p. 44.

<sup>3</sup> Humphry, *Observations in Myology*, 1872; *The Muscles of Lepidosiren annectens with the Cranial Nerves*, pp. 77 and 79.

<sup>4</sup> Gegenbaur, *Hexanchus*, p. 549.

eye-muscle nerves are represented by branches of the fifth; which, however, is a wider and more positive statement than Hyrtl really made.

Stannius<sup>1</sup> also states, on Hyrtl's authority, that the eye-muscle nerves have no independent roots; and Huxley<sup>2</sup> notes that in *Lepidosiren* "the three motor nerves of the eyeball are completely fused with the ophthalmic division of the fifth," a condition which he is disposed to view as the most primitive arrangement met with among vertebrates.

In considering what importance is to be attached to this often-quoted exception to the general rule, we have first to notice that we are dealing with animals in which the eyes are "very small" and "feebly developed;" secondly, that the eye-muscles are so small that their very existence was not only overlooked, but expressly denied, by so competent an anatomist as Prof. Owen; thirdly, that the two anatomists, Hyrtl and Humphry, who have described these muscles, agree in saying that the *recti* muscles are alone present, a condition clearly not fully realised by those who state, on Hyrtl's authority, that the fourth nerve is, like the third and sixth, represented by a branch of the fifth; fourthly, that in neither of the cases mentioned were the nerves actually traced into the muscles in question.

To these points we must add one, urged with great force by Schwalbe,<sup>3</sup> and which acquires much weight from the cases of *Petromyzon* and *Lepidosteus* already considered, viz., that a sufficiently careful examination of the brain has not been made to render us certain as to the alleged absence of independent roots of origin for such of the eye-muscle nerves as may be present.

The importance of Schwalbe's warning is strikingly exemplified by the recent observations of Wiedersheim<sup>4</sup> on the nervous system of *Lepidosiren* (*Protopterus*) *annectens*. Wiedersheim describes a moderately long but exceedingly slender nerve which leaves the skull through a special foramen in front of that of the fifth, and loses itself in the eye-muscles in a manner

<sup>1</sup> Stannius, *Das peripherische Nervensystem*, p. 18.

<sup>2</sup> Huxley, *Anatomy of Vertebrated Animals*, p. 73, note.

<sup>3</sup> Schwalbe, *Das Ganglion Oculomotorii*, p. 72.

<sup>4</sup> Wiedersheim, *Morphologische Studien*, Heft 1; III. *Das Skelet und Nervensystem von Lepidosiren annectens*, 1880.

he was unable to determine with certainty. In spite, however, of taking "all conceivable pains," he was unable to ascertain whether this hitherto overlooked eye-muscle nerve arises independently from the brain, or is a mere branch of the fifth, though he is inclined himself to regard it as an independently arising third nerve.

Under these circumstances, and especially when we consider Wiedersheim's discovery of a distinct eye-muscle nerve, and his statement of the extreme difficulty he experienced in tracing this nerve even to the limited extent which he succeeded in doing, we must, I think, conclude that, whatever subsequent investigation may tell us, *Lepidosiren* at present offers no definite or reliable evidence against the statement that the eye-muscle nerves are independently arising nerves in all vertebrates in which the eye-muscles themselves are present.

6. *Amphibia*.—Statements of exceptional innervation of one or more of the eye-muscles among *Amphibia* are by no means uncommon; and though I have devoted some time to making my list as complete as possible, I am far from certain that I have succeeded in collecting all the alleged cases. The following list includes all I have been able to refer to, and certainly all that are mentioned in the standard works and papers on the subject:—

A. *Apoda* (*Gymnophiona*).—Wiedersheim, in his monograph on this group,<sup>1</sup> mentions that in *Cecilia* the eye-muscles are present, but of exceedingly small size, so small indeed that he could not make out either their number or arrangement; neither was he able to ascertain anything concerning their innervation; indeed, he makes no mention whatever of the eye-muscle nerves. Fischer<sup>2</sup> also failed, from his dissection of a single specimen, to make out anything definite concerning the eye-muscle nerves. Inasmuch as the eyes of *Cecilia* are very small, it would seem probable that we have here another instance of rudimentary eyes, accompanied very possibly by reduction in the number of eye-muscles; and we have already seen that the evidence yielded by such cases cannot be accepted as in any way affecting the question of the primitive independence of the eye-muscle nerves.

<sup>1</sup> Wiedersheim, *Die Anatomie der Gymnophionen*. Jena, 1879, pp. 55, 56, and 61.

<sup>2</sup> Fischer, *op. cit.* p. 47.

B. *Caudata* (*Urodela*).

(a) *Proteus*.—The specimen of *Proteus* dissected by Fischer<sup>1</sup> was, like that of *Cecilia*, too imperfectly preserved to permit him to make any positive statement concerning the eye-muscle nerves; indeed, he calls attention to and expressly regrets his inability to determine whether these nerves are present or absent. The eyes of this cave-dwelling amphibian are situated beneath the skin, and are of very rudimentary structure, being arrested at what is in other vertebrates a very early embryonic condition.<sup>2</sup> As has been pointed out by Schwalbe,<sup>3</sup> Fischer does not in any way deny the existence of eye-muscle nerves, but merely records his inability to find them in a very imperfectly preserved specimen.

(b) *Salamandra* and *Triton*.—I take these two genera somewhat out of their proper zoological order, because they afford perhaps the most widely-known and frequently-quoted examples of abnormal innervation of the eye-muscles—instances which must accordingly be carefully considered.

Fischer, who was the first to draw attention to the point,<sup>4</sup> states that in *Salamandra* and *Triton* the third nerve, though rising independently from the brain, only supplies three of the eye-muscles—the *rectus internus*, *rectus inferior*, *obliquus inferior*—the *rectus superior* receiving a special branch from the “nasal division” of the fifth, which branch is absent in *Anura* in which the innervation is normal. In discussing the importance of this, he says:—“*Quid igitur veri possit esse similis, quam quod partium duarum, in quas penes Salamandrina divisum sit oculomotorius, altera eandem, quam in Ecaudatis retinuerit formam, altera cum Trigemino se conjunxerit?*” The fourth nerve in the same two genera, according to Fischer, “seems to have coalesced with the fifth pair;” at any rate, he was unable to discover any independent nerve, and the *obliquus superior* muscle is supplied by the “nasal branch” of the fifth. The sixth nerve is perfectly normal both in its origin and distribu-

<sup>1</sup> Fischer, *op. cit.* pp. 35 and 47.

<sup>2</sup> For a description and figure of the eye of *Proteus*, vide Semper, *Animal Life*, International Science Series, pp. 78, 79.

<sup>3</sup> Schwalbe, *Das Ganglion Oculomotorii*, p. 72.

<sup>4</sup> Fischer, *op. cit.* pp. 24, 25, 32, and 47.

tion; it passes very close to the Gasserian ganglion, but is really distinct from it, and leaves the skull by an aperture distinct from that of the fifth.

Fischer's careful descriptions, which have the great advantage of being illustrated by as careful figures,<sup>1</sup> have been referred to by many writers—Stannius,<sup>2</sup> Gegenbaur,<sup>3</sup> Hoffmann,<sup>4</sup> Stieda,<sup>5</sup> &c.—who, however, have added nothing to our knowledge on the subject from direct observations of their own.

Schwalbe,<sup>6</sup> who appears to be the only anatomist since Fischer's time who has directly investigated this interesting point, has furnished additional information of great value concerning it. He finds, in confirmation of Fischer's statement, that the nerve to the *rectus superior* muscle is derived, not from the third nerve, but from the "nasal branch" of the fifth; but points out that before this nerve is given off the third and nasal nerves cross and lie in very close contact with one another. He considers it probable that at this point there is direct connection between the two, although he was unable to prove it; and he accordingly supports the view, held also by Fischer and Stannius, that the supply of the *rectus superior* by the fifth is only apparent and due to the close connection and partial fusion of the third and fifth nerves at this point of crossing.

Concerning the fourth nerve, Schwalbe's results are more positive, and of great importance. He finds that although in the majority of specimens of *Salamandra maculosa* he dissected the arrangement described by Fischer obtained, the nerve to the *obliquus superior* appearing as a branch of the nasal nerve, yet that in some cases, one of which he figures,<sup>7</sup> the fourth may be a completely independent nerve, arising from the brain in the normal position.

Reviewing, then, these much-quoted cases of *Salamandra* and *Triton*, we find that Fischer's account of the anatomical arrangement of the nerve is confirmed by Schwalbe. We find that the

<sup>1</sup> Fischer, *op. cit.*, tab. ii. fig. 2 (*Salamandra*), and fig. 3 (*Triton*).

<sup>2</sup> Stannius, *Das peripherische Nervensystem*, p. 19.

<sup>3</sup> Gegenbaur, *Hexanchus*, p. 549, note 1.

<sup>4</sup> Hoffmann, *Bronn's Thierreich*, Bd. vi. heft. ii. *Amphibia*, p. 204.

<sup>5</sup> Stieda, *loc. cit.* p. 174.

<sup>6</sup> Schwalbe, *Das Ganglion Oculomotorii*, pp. 25–27.

<sup>7</sup> Schwalbe, *Das Ganglion Oculomotorii*, Tab. xiii. fig. 13.

sixth nerve is perfectly independent both at its root and along its whole course—is, in fact, in every way normal. That the fourth nerve is, as a rule, an apparent branch of the “nasal branch” of the fifth, but, at least in *Salamandra*, may be not uncommonly an independent nerve, normal in every respect. That the third nerve always arises independently from the brain; that it crosses the “nasal branch” of the fifth, lying in close contact with it as it does so; and that it supplies only three muscles—the *rectus internus*, *rectus inferior*, and *obliquus inferior*—the *rectus superior* receiving its branch from the “nasal nerve,” and this branch coming off beyond the point of crossing of the third and nasal nerves; and that this condition of things is interpreted by both the writers, who have investigated it directly—Fischer and Schwalbe—as merely implying that the third nerve has become partially fused with the fifth.

Concerning this “nasal nerve,” from which, in the two genera in question, the branch to the *rectus superior* always, and that to the *obliquus superior* usually, arises, there is a further point of importance. Schwalbe<sup>1</sup> has attempted to prove that this “nasal nerve” really corresponds, in part at least, to the *ramus ophthalmicus profundus* of Selachians. The point could only be decided by a study of the development of this nerve in *Urodela*, of which at present we know nothing; but should Schwalbe prove to be correct, the very slight amount of deviation from the normal condition which we have found to be all that really occurs in *Salamandra* and *Triton* would be still further reduced; for embryology teaches us that the *ramus ophthalmicus profundus* of Selachians is really a connecting branch between the third and fifth nerves, which cannot be said to belong distinctly to either one or the other, and that the portion of this nerve beyond the point at which it crosses the third nerve, from which portion we have seen that the branch to the *rectus superior* arises, has nothing whatever to do with the fifth, but belongs really to the third nerve.<sup>2</sup>

From what has been said above, I think that no other conclu-

<sup>1</sup> Schwalbe, *Das Ganglion Oculomotorii*, p. 26.

<sup>2</sup> Marshall, “Head Cavities and Associated Nerves of Elasmobranchs,” *Quart. Journ. of Micros. Science*, January 1881, p. 89; and Marshall and Spencer, “Cranial Nerves of Scyllium,” *Quart. Journ. of Micros. Science*, July 1881, pp. 494 *seq.*

sion can be drawn than that the cases of *Salamandra* and *Triton* do not afford any reason for regarding the eye-muscle nerves as other than independent and constant nerves.

(c) *Menobranchus*.—Gegenbaur<sup>1</sup> states, on Fischer's authority, that in *Menobranchus*, as in *Salamandra* and *Triton*, the fourth nerve is replaced by a branch of the fifth. I have been unable to refer to Fischer's account, so that any discussion of the case would be unprofitable. It is, however, very possible that the condition is really what Schwalbe has shown to occur in *Salamandra*.

(d) *Siredon*.—Fischer<sup>2</sup> has established that the third and fourth nerves are normal in origin and distribution, but was unable to make out anything definite concerning the sixth nerve.

(e) *Cryptobranchus*.—Schmidt, Goddard, and V. d. Hoeven are quoted by Hoffmann<sup>3</sup> as stating that in the *Cryptobranch* the third and fourth are independent nerves, but that the sixth is a branch of the nasal division of the fifth.

Professor Humphry<sup>4</sup> remarks that the dissection of the cranial nerves is difficult, on account of the "tough areolar tissue of the animal and the numerous accompanying veins." He was unable to "discover the third, fourth, or sixth nerves in the orbit." The third and fourth were, however, found in the cranial cavity, but not the sixth.

Here, again, our information is too imperfect to allow definite conclusions to be drawn. If the sixth nerve really appears as a branch of the fifth, it is of importance to note that, as is evident from Professor Humphry's figure, the fifth and seventh nerves are quite distinct from one another—a point to which we shall refer when considering the *Anura*.

C. *Anura*.—The condition of the eye-muscle nerves in *Anura* has been carefully investigated by a number of anatomists, notably by Fischer<sup>5</sup> and Schwalbe.<sup>6</sup> The results of these investi-

<sup>1</sup> Gegenbaur, *Hexanchus*, p. 549, note 1.

<sup>2</sup> Fischer, *Anatomische Abhandlungen über die Perennibranchiaten und Dero-tremen*, Hamburg, 1864, p. 127.

<sup>3</sup> Bronn's *Thierreich*, Bd. vi.

<sup>4</sup> Humphry, *Observations in Myology*, p. 45, and pl. iv. fig. 22.

<sup>5</sup> Fischer, *Amphibiorum nudorum Neurologia, specimen primum*, pp. 8–22 and 45–48.

<sup>6</sup> Schwalbe, *Das Ganglion Oculomotorii*, pp. 28–31.



gations are as follows:—In all *Anura* that have been examined, the third and fourth are distinct and independent nerves, with normal origin and distribution. In *Pelobates* and *Bombinator* the third leaves the skull by the same foramen as the fifth, with which it is in very close contact, though the two nerves are really distinct.

The sixth nerve in all cases has an independent origin from the brain in the normal position. In *Bufo*,<sup>1</sup> the sixth nerve preserves its independence along its whole course, and is in all respects perfectly normal. In the other *Anura* examined—viz., *Pipa*, *Rana*, *Pelobates*, *Bombinator*, and *Hyla*—the sixth nerve, though arising independently, unites with the Gasserian ganglion, and the branch to the *rectus externus* is derived from the “nasal branch” of the fifth.<sup>2</sup>

But little criticism is called for by the above account. As was urged in the case of *Lepidosiren*, the presence of a distinct root of origin in the normal position must be held to prove that the sixth nerve is in the cases quoted above really an independent nerve, in spite of its apparent fusion with the fifth at the Gasserian ganglion. The fact that the sixth in an allied genus (*Bufo*) retains its independence, is an additional argument in favour of the fusion being secondarily acquired; and this view must be considered to be established by the statement made by Stannius,<sup>3</sup> on Fischer's authority, that the sixth nerve is independent of the fifth in the larval stages of those forms which, when adult, have the two nerves fused.

This concludes the list of recorded instances of exceptional innervation of the eye-muscles. Leaving out, as we are fairly entitled to, the cases of *Amphioxus* and of those forms in which, as in *Amblyopsis*, the eyes are rudimentary and functionless; the results of an examination of the remaining instances may be stated thus:—

<sup>1</sup> Fischer, *op. cit.* p. 5, and tab. ii. fig. 1.

<sup>2</sup> Cf. Fischer, *op. cit.* pp. 3–22, and tab. i. fig. 2 (*Hyla*) fig. 3 (*Bombinator*), fig. 4 (*Pelobates*); and tab. ii. fig. 1 (*Pipa*), fig. 4 (*Rana*); also Wyman, *Anatomy of the Nervous System of Rana pipens*, New York, 1853, pp. 26–28; also Wiedersheim in Ecker's *Anatomie des Frosches*, Zweite Abtheilung, 1881, pp. 20–21.

<sup>3</sup> Stannius, *Handbuch der Zoologie*, Zweites Buch, *Die Amphibien*, 1856, p. 150, note 3.

1. That in no single instance has it been established that any one of the eye-muscle nerves is replaced by a branch of the fifth, or of any other nerve—the cases in which this is alleged to occur being far more naturally explained by supposing partial fusion between the nerves concerned to have occurred.

2. That in the alleged cases of replacement of one or more of the eye-muscle nerves by a branch of the fifth nerve, the “branch of the fifth” in question is very probably the *ramus ophthalmicus profundus*, which is really a communicating nerve between the third and fifth, belonging as much to one as to the other in its posterior portion, and in its anterior part belonging exclusively to the third.

3. That the instances in which the absence of one or other of the eye-muscle nerves has been alleged are either, as in *Petromyzon*, *Lepidosteus*, *Pipa*, *Hyla*, &c., cases in which the nerves in question arise from the brain in a perfectly normal manner, and after running a certain distance within the skull become connected more or less intimately with the fifth nerve; or else cases in which, as in *Lepidosiren*, the eyes are small, the eye-muscles imperfectly developed, and the descriptions of their anatomy incomplete and unsatisfactory.

4. That such cases do not in any way invalidate the proposition that the third, fourth, and sixth are independent nerves throughout the vertebrate sub-kingdom.

I propose now to consider briefly the leading features exhibited by the eye-muscle nerves individually.

III. **The Third, or Oculomotor Nerve.**—Since the third nerve is found to be an independent nerve throughout the vertebrate series, it becomes of interest to inquire whether or not it possesses segmental value.

Observations by different investigators during the last few years have tended very strongly to support, if, indeed, they may not be said to have established, the claim of the third nerve to rank among segmental nerves. Inasmuch as this point has been very fully discussed recently<sup>1</sup> I do not propose to go over the

<sup>1</sup> Marshall, “Development of Cranial Nerves in Chick,” *Quart. Journ. of Micros. Science*, January 1878, pp. 23–27; and “Head Cavities and Associated Nerves of Elasmobranchs,” *Quart. Journ. of Micros. Science*, January 1881, pp. 78–83; also Schwalbe, *Das Ganglion Oculomotorii*.

whole of the evidence here, but shall merely apply, in a somewhat summary manner, the several tests of segmental value in the order given on a previous page.<sup>1</sup>

1. Though the earliest stages of development of the third nerve have not yet been ascertained with precision in any case, yet there is very strong reason for thinking that in the chick, at any rate, the third nerve develops, like the hinder cranial nerves and the posterior roots of the spinal nerves, as an outgrowth from the neural crest on the top of the mid-brain.<sup>2</sup>

2. Inasmuch as, at a rather later, though still early period—about the sixtieth hour in the chick, and stage K of Balfour's nomenclature in the dog-fish—the third nerves arise from the base of the mid-brain, very near the mid-ventral line, it is clear that, if the observations on the earlier stages are correct, the roots must shift downwards at an early period, and to an extent unequalled by any other nerve.

Kölliker has described the later stages of this shifting, as seen in rabbit embryos, as follows:<sup>3</sup>—In an embryo 12 days 5 hours old, and 7 mm. long, the third nerve arose from the mid-brain, not from its ventral surface, but about half-way up its side; later on it shifts ventralwards, "like the ganglionated cranial nerves and the sensory spinal roots," being found on the ventral surface of the mid-brain in an embryo of the 14th day, and 15 mm. long.

3. The course of the main stem of the nerve is (fig. 8, IV.) at right angles to the axis of the head at the point of origin of the nerve.

4. Morphologists are very far from agreeing as to the existence of a visceral cleft in front of the mouth, so that it would be premature to discuss the relations of the third nerve to this "lachrymal cleft," for whose existence there is, however, much to be said. Concerning the head cavities, however, the evidence yielded by the third nerve is of a perfectly definite and convincing character. The nerve in Elasmobranchs passes downwards and backwards from its root of origin to the interval

<sup>1</sup> *Supra*, p. 313.

<sup>2</sup> Cf. Balfour, *Comparative Embryology*, vol. ii. p. 379.

<sup>3</sup> Kölliker, *Entwicklungsgeschichte des Menschen und der höheren Thiere* Zweite Auflage, 1879, p. 613.

between the dorsal ends of the first and second head-cavities, where it expands into a ganglionic swelling—the ciliary ganglion. Beyond this point the main trunk of the nerve passes down between the two cavities, *the relations of the third nerve to the first and second cavities being precisely the same as those of the fifth nerve to the second and third cavities.*<sup>1</sup>

5. As just noticed, there is a very evident ganglionic swelling at the point of division of the third nerve into its two main branches.

These considerations are, I think, when taken in conjunction with its previously established constancy throughout the vertebrate series, sufficient to establish the proposition that the third nerve is of segmental nature. The further question, whether the third represents an entire segmental nerve, or only a portion of one, will be best answered by considering the fourth nerve.

**IV. The Fourth or Trochlear Nerve.**—Having established the constancy of this nerve, we have now to consider its morphological import. Concerning its development we know very little, but that little is of importance. In the dog-fish it has been shown<sup>2</sup> that the fourth nerve, at the earliest period at which it has been recognised, arises from the brain at the same spot as in the adult, *i.e.*, the dorsal surface of the hinder end of the mid-brain; further, that its course is from the first that of a segmental nerve.

Now, if the visceral clefts and arches, and the head cavities give us, as they most certainly do, reliable clues as to the segmentation of the head, then it is seen at once *that there is no room for a segmental nerve between the third and fifth nerves; and therefore, if the fourth is of segmental nature, it must belong to one or other of these nerves.*

The following considerations seem to point very strongly to the third and fourth nerves being connected together, and favour the view that they are together equivalent to a segmental nerve.

1. The two nerves in question, the third and fourth, both

<sup>1</sup> Marshall, "Head Cavities and Associated Nerves of Elasmobranchs," *Quart. Journ. of Micros. Science*, Jan. 1881, pp. 78 *seq.*

<sup>2</sup> Marshall and Spencer, "Cranial Nerves of Scyllium," *Quart. Journ. of Micros. Science*, July 1881, pp. 672-674.

arise from the mid-brain or middle cerebral vesicle. Furthermore, they are the only nerves that arise from this division of the brain, either in the embryo or the adult. There are independent reasons for thinking that these brain-vesicles have segmental value;<sup>1</sup> and though these reasons may not be considered conclusive on the point, they nevertheless lend some support to the view that the two nerves arising from one of these vesicles belong to the same segment.

2. The third and fourth nerves, though arising separately from the brain, may be connected together more or less intimately beyond their roots of origin. This, for instance, is a marked feature both in *Petromyzon* and *Lepidosteus*, also in *Salamandra* and *Triton*, if Schwalbe is correct in identifying the "nasal branch" of the fifth with which both the third and fourth nerves are connected as the *ramus ophthalmicus profundus*.

3. According to Meynert,<sup>2</sup> the third and fourth nerves arise in the adult from a common nucleus. This has, however, been denied by Forel,<sup>3</sup> though supported by other investigators, and probably requires confirmation.

4. The fourth, though chiefly known as a motor nerve, is really in many animals a nerve of mixed function, giving off in *Selachians* and *Amphibians*<sup>4</sup> sensory branches to the conjunctiva and skin of the upper eyelid. This point is of importance, because if the third and fourth are together equivalent to a segmental nerve, it would be only reasonable to expect that certain of its fibres should be sensory; and analogy would certainly lead us to look for sensory branches in the portion with the more dorsally situated root, *i.e.*, the fourth nerve, which, as we have just seen, does actually present such sensory fibres.

5. That the fourth nerve is itself not an entire segmental nerve is rendered probable by the fact, noticed by Schwalbe, that it has no ganglion, and is strongly supported by the further fact

<sup>1</sup> Vide Foster and Balfour, *Elements of Embryology*, part i. p. 183; and Marshall, *Development of Nerves in Birds*, this *Journal*, vol. xi. p. 510.

<sup>2</sup> Meynert, "The Brain of Mammals," *Stricker's Histology*, New Sydenham Society's Translation, vol. ii. pp. 444, 445.

<sup>3</sup> Forel, *Haubenregionen*.

<sup>4</sup> Schwalbe, *Das Ganglion Oculomotorii*, p. 14; Wiedersheim, *Morphologische Studien*, p. 21; and in Ecker's *Anatomie des Frosches*, p. 24; also Hoffmann, *Bronn's Thierreich*, Bd. vi. p. 203.

that the third nerve almost certainly arises at first from the dorsal surface of the brain, and beyond all doubt is, during its early stages, attached much higher up the side of the brain than it is at a later stage, *i.e.*, that the third nerve behaves like a posterior spinal root.

Since, as we have seen, there is no room for a separate segmental nerve between the third and the fifth, I am inclined to view the third and fourth nerves as together equivalent to a segmental nerve, which has divided into two portions, whereof one—the fourth—has remained in its primitive position on the top of the brain, while the other—the third—has, like the other cranial nerves and the posterior spinal roots, shifted downwards, the extent of the shifting being greater than that of any of the other nerves, but the several steps of the process probably the same as in these. This view will be found to be very closely in accordance with that advocated by Schwalbe.<sup>1</sup>

**V. The Fifth or Trigeminal Nerve.**—It will be convenient to continue the consideration of the cranial nerves in the usual sequence, and to take the remaining eye-muscle nerve—the sixth—after the trigeminal.

The fifth nerve completely fulfils all the conditions of a true segmental nerve.<sup>2</sup> It appears very early as an outgrowth from the neural crest. The root of origin from the brain shifts down at an early period, acquires a secondary attachment to the side of the brain, and loses its primary attachment completely. The direction of the main stem is at right angles to the axis of the head at the point of origin of the nerve. The maxillary and mandibular branches are related to the maxillo-mandibular or buccal cleft in the manner characteristic of the posterior segmental nerves, as was first pointed out by Stannius. The relations of the fifth nerve to the second and third head-cavities are of a perfectly typical nature; and finally a ganglionic

<sup>1</sup> Schwalbe, *Das Ganglion Oculomotorii*, pp. 77, 78.

<sup>2</sup> For the development of the fifth nerve in Elasmobranchs, *vide* Balfour, *Elasmobranch Fishes*, 1878, p. 196-198; also Marshall and Spencer, "Observations on the Cranial Nerves of Scyllium," *Quart. Journ. of Micros. Science*, July 1881, pp. 474-479; in the Chick, *vide* Marshall, *Quart. Journ. of Micros. Science*, Jan. 1878, pp. 28-32; and in the Rabbit, Kölliker, *Entwicklungsgeschichte*, 1879, pp. 610-712.

enlargement—the Gasserian ganglion—is present in the nerve a short distance above its division into the two main branches.

The only possible doubt as to the independent segmental value of the fifth nerve hinges on the fact that in the two lower classes of vertebrates—*Pisces* and *Amphibia*—the fifth is very generally fused more or less completely with the seventh in the adult condition; the fusion sometimes, as in most fishes, involving the roots to a greater or less extent, sometimes, as usually in *Amphibians*, occurring a short distance beyond the roots and close to the Gasserian ganglion.

This approximation or fusion of the fifth and seventh nerves has, as mentioned above, been employed by J. Müller, Stieda, and others, as an argument against the two nerves being of independent segmental value.

A crucial test of the force of this argument is afforded by a study of the development of the roots of the two nerves in Elasmobranchs, in which the fusion of the roots in the adult is so complete that what is really one of the roots of the seventh has hitherto been almost invariably described by anatomists as a root of the fifth.<sup>1</sup> In the dog-fish it has been shown that the two nerves, though so intimately connected in the adult, are in the early embryonic stages perfectly distinct from one another, and some distance apart, as far from one another, indeed, as they are in corresponding stages of such forms as the chick or lizard in which they remain completely separate throughout life; and that the gradual approximation and fusion of the two nerves, which occur during the later developmental stages, all the steps of which have been traced, must, like the partial fusion which we have seen may occur in some forms between the third and fifth nerves, be viewed as purely secondary features.

In early stages of both *Teleostean*s and *Amphibians*, I have also noticed that the roots of the fifth and seventh nerves are perfectly distinct from one another, and some distance apart, and that their subsequent approximation must accordingly be, as in Elasmobranchs, of a purely secondary nature.

<sup>1</sup> A full account of the development of the roots of the fifth and seventh nerves in the dog-fish, and of the relation of the embryonic to the adult roots, will be found in the paper by Mr. Spencer and myself quoted above, *Quart. Journ. of Micros. Science*, July 1881, pp. 482-486.

The claim of the fifth nerve to rank as an independent segmental nerve must, I think, from what has been said above, be considered as definitely established.

**VI. The Sixth or Abducent Nerve.**—The proper morphological position of this nerve is by no means easy to determine with any degree of certainty; and the views of different writers on the point are far from being in harmony with one another.

In a former section of this paper we have established the fact that the sixth is an independent nerve throughout the vertebrate sub-kingdom. It always supplies the *rectus externus* muscle of the eyeball, and may supply other parts as well; thus, in reptiles it supplies the retractor muscle of the bulb of the eye, and in *Batrachia* the suspensor muscle of the bulb and the muscles of the nictitating membrane.<sup>1</sup> *In all cases it is a purely motor nerve.* Indeed, if we omit the eleventh and twelfth pairs, which are not constant cranial nerves, the sixth is not only the most purely motor cranial nerve, but the only exclusively motor one throughout the vertebrate series.

Its point of origin from the brain in adult vertebrates is also a remarkable and constant one. It arises from the under surface of the medulla, very close to the mid-ventral line, and vertically below, or more usually slightly posterior to the common root of origin of the seventh and eighth nerves. In some cases the root may be in front of that of the seventh nerve. The root is always slender, and devoid of ganglion cells.

Concerning the development of the sixth nerve, we unfortunately know but little. At the fifth day in the chick,<sup>2</sup> and at a corresponding stage in the dog-fish,<sup>3</sup> it has been detected and described, its appearance and relations being practically identical in the two cases. It is a slender nerve, with no ganglion cells at any point in its length, arising from the ventral surface of the hind-brain, below the seventh nerve, by

<sup>1</sup> Stannius, *Handbuch der Zoologie*, Zweite Auflage, Zoologie der Amphibien, 1856, p. 150.

<sup>2</sup> Marshall, "Development of Cranial Nerves of Chick," *Quart. Journ. of Micros. Science*, Jan. 1878, pp. 23-25.

<sup>3</sup> Marshall, "Head Cavities and Associated Nerves of Elasmobranchs," *Quart. Journ. of Micros. Science*, Jan. 1881, pp. 89-93.



a number of small slender roots, and running forward to the *rectus externus* muscle, in which it ends. The roots are from the earliest period at which the nerve can be recognised close to the median ventral line (fig. 7, VI.), and some distance below the root of the seventh (fig. 7, VII.), from which they are from the start perfectly distinct. So far as can be inferred from negative evidence, the sixth nerve appears to develop later than the seventh and other segmental nerves.

From the above account it is clear that the sixth has no claim whatever to segmental rank, inasmuch as it distinctly fails to answer to any one of the tests of such rank laid down on page 313. It does not develop from the neural crest. The roots of origin do not shift downwards, but are from their first appearance in the adult position. The course of the nerve is nearly parallel to, and certainly not perpendicular to the axis of the head. It has not the definite relations to the visceral clefts and arches, and to the head cavities, characteristic of a segmental nerve. And it has no ganglion cells at any point in its length.

As the nerve is not an independent segmental nerve, it must either belong to one of the segmental nerves or else be a nerve of altogether exceptional nature. The latter supposition should, I think, only be adopted as a last resource if all the other attempts at explanation fail, and I therefore propose now to consider the relations of the sixth to the segmental nerves, or rather to the fifth and seventh nerves, which are clearly the only ones which could claim it.

By the majority of writers who have discussed this point, the sixth is referred to the fifth. Thus, Gegenbaur considers the sixth to be an independently arising motor root of the fifth, a view which Schwalbe<sup>1</sup> also adopts. Wiedersheim<sup>2</sup> speaks of the fifth and sixth nerves as together making up a segmental nerve; while Huxley<sup>3</sup> is disposed to view the sixth as primarily part of the fifth.

Notwithstanding the weight of authority against me, I think that the sixth nerve should be grouped with the seventh, and not with the fifth, for the following reasons:—

<sup>1</sup> Schwalbe, *Das Ganglion Oculomotorii*, p. 74.

<sup>2</sup> Wiedersheim, *Morphologische Studien*, p. 28.

<sup>3</sup> Huxley, *Anatomy of Vertebrated Animals*, p. 78, note

1. In the early stages of both chick and dog-fish the roots of the sixth are completely behind those of the fifth nerve. Indeed, the majority of the roots are even behind the roots of the seventh; and although a transverse section may, as in fig. 7, pass through the roots of both sixth and seventh nerves, yet the root of the sixth in such a section is the most anterior of the series, the other roots being further back, and completely behind the seventh root.

2. In adult vertebrates, also the sixth nerve usually arises beneath or slightly behind the seventh, very rarely in front of it.

3. Though the sixth nerve may, beyond its root, be closely connected with the fifth, yet it is important to notice that *all the cases*—*Petromyzon*, *Lepidosiren*, *Pipa*, *Rana*, and other *Anura*—in which it is described as fusing with the fifth, are also cases in which the seventh and fifth nerves are very closely connected together, so that the connection between the sixth and fifth in these instances by no means proves that the sixth belongs to the fifth, but is more probably due to the same cause—whatever it may be—that determines the approximation or fusion of the seventh and fifth nerves.

Concerning the actual value of the sixth nerve, I see no reason to alter the opinion I have previously expressed, that the sixth nerve may be regarded as having the same relation to the seventh that the anterior root of a spinal nerve has to its posterior root. I shall return to this point when considering the seventh nerve.

**VII. The Seventh or Facial Nerve.**—As to the segmental value of the seventh nerve there can be no doubt whatever; for, like the fifth, it completely and indisputably fulfils all the conditions of a segmental nerve laid down on page 313.

It develops<sup>1</sup> very early as an outgrowth from the neural crest on the dorsal surface of the hind-brain (fig. 5); at an early stage the nerve acquires a new or secondary attachment to the side of the brain (fig. 6); but, unlike all the other nerves, cranial or

<sup>1</sup> For an account of the development of the seventh nerve in *Elasmobranchs*, vide Balfour, *Elasmobranch Fishes*, 1878, pp. 198–202; and Marshall and Spencer, *Quart. Journ. Micros. Science*, July 1881, pp. 679–691; in the Chick, vide Marshall, *Quart. Journ. Micros. Science*, Jan. 1876, pp. 34–36; and in Mammals, Kölliker, *Entwickelungsgeschichte*.

spinal, the original or primary root is retained as well as the secondary root; whereas in all the other nerves the primary attachment appears to be lost (figs. 3 and 4). The general course of the nerve is at right angles to the axis of the head at its point of origin; the relation of its branches to the hyo-mandibular cleft, first pointed out by Stannius, and afterwards insisted on by Gegenbaur, are those of a typical segmental nerve, as are its relations to the head cavities; whilst, finally, it is ganglionic at its division into the two main ventral branches.

As to the independent rank of the seventh nerve, I have already discussed fully the theory that the seventh and fifth nerves are connected together primarily, and have stated the arguments leading to the conclusion, that although in many vertebrates—fishes and amphibians—the two nerves are more or less closely fused together, yet that embryology shows that this fusion is a secondarily acquired character.

The relation between the sixth and seventh nerves is of still greater importance, from its bearing on the disputed question of whether there are to be found in any of the cranial nerves roots strictly comparable with the anterior roots of the spinal nerves.

In dealing with this question, it is first necessary to establish certain general conclusions concerning the cranio-spinal nerves. As was first pointed out by Balfour, the posterior roots of the spinal nerves must be regarded as of a more primitive nature than the anterior roots, the grounds on which this conclusion is based being the following:—

1. The actual mode of development of the two kinds of roots in the spinal nerves. As noticed in a previous page,<sup>1</sup> the posterior roots appear before the anterior ones, and are also in their mode of development of a more primitive character than these latter, the posterior roots consisting at first entirely of undifferentiated spherical or polygonal cells, while the anterior roots are almost from their first appearance fibrillar.

2. The condition of the nervous system in *Amphioxus*, in which, as conclusively shown by Balfour,<sup>2</sup> all the nerves arise by single roots, which roots correspond to the dorsal or posterior roots of other vertebrates, and which must clearly in *Amphioxus* be of mixed motor and sensory function.

<sup>1</sup> *Supra*, p. 312.

<sup>2</sup> Balfour, *Elasmobranch Fishes*, p. 193.

From these facts the further conclusion is drawn "that primitively the cranio-spinal nerves of vertebrates were nerves of mixed function with one root only, and that root a dorsal one; and that the present anterior or ventral root is a secondary acquisition."<sup>1</sup>

Concerning the several steps by which these anterior roots have been acquired, the evidence at our disposal is of an imperfect, and in great part merely conjectural character. Still I think that, though we may not be able to solve the problem completely, we can at any rate define its limits fairly accurately, and perhaps indicate the path along which the solution will ultimately be found.

The problem is how, from animals resembling *Amphioxus* in possessing only dorsal roots to the nerves, and these dorsal roots consequently of mixed function, has the type of spinal nerve met with among existing vertebrates, with two distinct roots, dorsal or sensory and ventral or motor, been derived?

It appears to me that there are two ways in which we can conceive this change as having come about:—

Firstly, we might suppose that in some way, and for some reason, the sensory and motor portions of the originally single root became completely separated from one another, and that while the sensory portion of the nerve retained the primitive mode of development and position of attachment of the root, the motor portion acquired a new mode of development and a new position of attachment, and then united farther on with the posterior root to form a spinal nerve. On this view the motor and sensory roots of a spinal nerve correspond to the motor and sensory portions of the single root of *Amphioxus*.

Or, secondly, we might imagine the anterior root to be, not the motor portion of the original root, but an altogether new development, an independent outgrowth from the spinal cord to supply the more complicated system of muscles that would necessarily accompany the gradual perfection and complication of the internal skeleton; that this new root was at first completely independent of the original or dorsal root, and for a time coexisted with a dorsal root of mixed function; that in the case of the spinal nerves the whole motor function gradually got transferred to, or usurped by, the new root; while the two roots,

<sup>1</sup> Balfour, *Elasmobranch Fishes*, p. 193.

originally separate along their whole length, became united to form the mixed trunk of the spinal nerve.

Now, although there are very considerable and obvious difficulties in the way of accepting either of these alternatives, yet it appears to me that the second is far more in accordance with the actual facts than the first, and that it offers a ready explanation of many points unintelligible on the first hypothesis. Thus, the second view explains why in actual development the anterior spinal roots appear later than the posterior, and why they are for some time quite distinct from these latter; it also explains such cases as *Petromyzon*, in which the anterior and posterior roots of the spinal nerves are said to remain distinct from one another throughout life.

By far the most important argument, however, in favour of the second hypothesis is afforded by the explanation it yields of the condition of the cranial nerves as compared with the spinal; and in connection with this point I would direct special attention to the statements already made concerning the sixth and seventh nerves.

It has been shown above that the seventh nerve in Elasmobranchs develops in a manner precisely similar to the posterior roots of the spinal nerves; that it arises as an outgrowth from the neural crest (figs. 5, VII.), the nerves of the two sides being at first directly and widely continuous with one another across the top of the brain; that by growth of the mid-dorsal roof of the brain the two nerves get separated from one another (figs. 6, VII.); that the root acquires a secondary attachment to the side of the brain (figs. 7, VII.), *but that, unlike the other cranial or the spinal nerves, it retains the primary as well as the secondary root throughout life. In this respect the seventh is, with the possible exception of the fourth, the most primitive nerve in the body, inasmuch as it exists throughout life in a condition which is only a transitory one in all the other nerves. However unexpected this point may be, I cannot but think that it is one of the greatest importance in the determination of any question concerning the morphology of the cranial and spinal nerves.*

The seventh being a very primitive nerve, there is strong *a priori* reason for thinking that the sixth nerve, which we have seen reason for grouping with the seventh, is also of a primitive

nature, and it is clear that on the second hypothesis such is the case, the complete independence of the sixth nerve being merely the retention of a primitive character, while its limited and special distribution to muscles not present in *Amphioxus* affords a very possible explanation of its appearance in higher vertebrates. On the first hypothesis, on the other hand, the sixth nerve would be, not a root which *had retained its primitive independence of the seventh*, but a root which had as a perfectly exceptional occurrence *acquired independence*, a view directly contradicted by the primitive condition of the seventh itself.

It must surely be regarded as a very significant fact that a transverse section through the hind-brain of *either an embryo or adult* Elasmobranch passing through the roots of the sixth and seventh nerves (fig. 7) agrees absolutely in all essential points with a section *at an early embryonic stage* through the roots of a spinal nerve in the same animal (fig. 3), *i.e.*, that a condition which is transitory in the case of the spinal nerves is permanently retained in the case of the sixth and seventh nerves. This fact, which is the strongest possible argument in favour of the second hypothesis, clearly directly contradicts the first.

If the doctrine that the cranial nerves are more primitive than the spinal appear at first sight paradoxical,<sup>1</sup> I would point out that there is independent evidence in favour of the head retaining a more primitive condition than the body. Thus the skull, though subjected to very extensive secondary modifications, is really in a more primitive state than the vertebral column, for the skull represents the permanent retention of a condition, that of a continuous unsegmented cartilaginous tube, which is only transitory in the case of the vertebral column except in the lowest vertebrates; the division of the cartilaginous tube into segments or vertebrae never occurring, and in all probability never having occurred in the skull, though so constantly present in the vertebral column. The fact that it is in the lowest vertebrates alone that this unsegmented condition is retained in the trunk

<sup>1</sup> I have myself on a former occasion both felt and urged this objection ("Head Cavities of Elasmobranchs," *Quart. Journ. of Micros. Science*, Jan. 1881, p. 91). Further investigation has convinced me that I was then wrong, and that Balfour was right in considering (*Elasmobranch Fishes*, p. 193) the cranial nerves as more primitive than the spinal, though I do not agree with his conclusion that the cranial nerves have no anterior roots.

as well as in the head, is a strong argument in favour of the view that the head is really in a more primitive condition than the trunk as regards skeletal elements.<sup>1</sup>

On the second hypothesis, the mixed—motor and sensory—nature of the seventh nerve is explained as due, like the persistence of the primary root and the independence of the sixth nerve, to retention of the primitive condition, and the extreme variability presented by the relative importance of the sensory and motor functions of the seventh nerve in different vertebrates may help to render intelligible how the posterior spinal roots, which were originally of mixed function, have become converted into purely sensory roots.

If the hypothesis advanced above should prove correct, it would be only reasonable to expect that the posterior roots of the spinal nerves should in some exceptional cases be found to retain in part their primitive mixed character, and to coexist as mixed posterior roots with exclusively motor anterior roots. I am not aware of any such cases, or of the existence of any residual physiological phenomena that would support such a view, but would suggest that a direct investigation of the functions of the spinal roots in the lampreys, where the two roots are stated to remain distinct from one another throughout life, might conceivably lead to interesting results.

The application of the hypothesis to the remaining cranial nerves is sufficiently obvious from the accounts given of them. The main point of difficulty concerns the determination of the presence or absence of anterior motor roots to these nerves; and on this point I have no additional evidence beyond what I have already stated elsewhere.<sup>2</sup>

**VIII. The Eighth or Auditory Nerve.**—In all the forms in which the development of the auditory nerve has been ascer-

<sup>1</sup> The following quotation from Balfour, which I only became acquainted with after the above passage was written, strongly confirms this view :—"This development (of the skull) probably indicates that the basilar plate contains in itself the same elements as those from which the neural arches and the centra of the vertebral column are formed, but that it never passes beyond the unsegmented stage at first characteristic of the vertebral column."—*Comparative Embryology*, vol. ii. p. 467.

<sup>2</sup> Marshall, "Head Cavities and Associated Nerves of Elasmobranchs," *Quart. Journ. of Micros. Science*, Jan. 1881, pp. 91-93.

tained, it arises as part of the seventh nerve. Neither its development nor its anatomical relations afford the slightest ground for thinking it to be of segmental rank.<sup>1</sup>

**IX. The Ninth or Glosso-pharyngeal Nerve.**—Like the auditory, the ninth nerve can be disposed of very briefly, but for a directly opposite reason. Since Gegenbaur confirmed Stannius' account of its relations to the first branchial cleft, the claim of the glosso-pharyngeal to rank as an independent segmental nerve has been very generally admitted; and as the history of its development<sup>2</sup> shows that it conforms in all respects to the characters of a segmental nerve as defined on page 313, it would be superfluous to discuss in detail its now universally recognised claims to segmental value.

**X. The Tenth or Vagus Nerve.**—The tenth nerve stands in much the same position as the ninth, with the exception that while the glosso-pharyngeal is a single segmental nerve, the vagus, from its relations to a number of visceral clefts, must be considered as equivalent to an equal number of segmental nerves fused together. This was first pointed out by Stannius, and subsequently developed in much more detail by Gegenbaur; and since the publication by the latter of his memorable essay on the cranial nerves of *Hexanchus*, has been accepted almost universally as the true theory of the morphological value of the tenth nerve. It is only necessary to add here that the study of its development shows that it completely fulfils all the conditions required of segmental nerves.

Concerning the number of primitively separate segmental nerves fused together to form the vagus, we cannot speak positively. The greatest number of clefts supplied by it in vertebrates above *Amphioxus* is met with among the *Marsipobranchii* and in *Notidanus*, where it supplies the six posterior branchial clefts, and must therefore be equivalent to *at least* six segmental nerves.

<sup>1</sup> For the development of this nerve in Elasmobranchs, *vide* Balfour, *Elasmobranch Fishes*, p. 198; in the Chick, Marshall, "Development of Cranial Nerves in Chick," *Quart. Journ. of Micros. Science*, Jan. 1878, pp. 34-36.

<sup>2</sup> For the development of the gloss-pharyngeal and vagus nerves in Elasmobranchs, *vide* Balfour, *Elasmobranch Fishes*, pp. 202, *seq.*; in the Chick, Marshall, *loc cit.* pp. 36-39.



Whether this is the full number, however, is a point not yet decided.

**XI. and XII. The Eleventh or Spinal Accessory, and Twelfth or Hypoglossal Nerves.**—Neither of these nerves is constant as a cranial nerve throughout the vertebrate series, a fact which renders it very doubtful whether the claim of either of them to segmental value could be entertained. For this reason, and partly because I am at present engaged in investigating their development, about which we know as yet very little, I do not propose to deal further with them in the present paper. Forming, as they do, the connecting links between cranial and spinal nerves, they may be expected to yield valuable evidence concerning the validity of the hypothesis propounded above concerning the relations between these two groups of nerves.

**Summary.**—The conclusions arrived at concerning the segmental value of the cranial nerves may be expressed in a tabular form thus:—

Segment.	Nerve.	Visceral Cleft.	Visceral Arch.
1. Præoral.	I. Olfactory.	Olfactory.	
2. Do. }	III. Oculomotor, } IV. Trochlear. }	Lachrymal.	
3. Oral.	V. Trigeminal.	Buccal.	Maxillary.
4. Postoral. }	VII. Facial, } VI. Abducent. }	Spiracular or hyomandibular. }	Mandibular.
5. Do.	IX. Glosso-pharyngeal.	1st Branchial.	Hyoid.
6. Do.	X. Vagus, 1st branch.	2nd „	1st Branchial.
7. Do.	„ 2nd „	3rd „	2nd „
8. Do.	„ 3rd „	4th „	3rd „
9. Do.	„ 4th „	5th „	4th „
10. Do.	„ 5th „	6th „	5th „
11. Do.	„ 6th „	7th „	6th „

**List of chief Works and Papers referred to, arranged  
according to date of publication.**

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- ... WIEDERSHEIM.—*Die Anatomie der Gymnophionen*.
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1881. BALFOUR.—*Treatise on Comparative Embryology*, vol. ii.

1881. MARSHALL.—On the Head Cavities and Associated Nerves of Elasmobranchs. *Quart. Journ. of Micros. Science*, vol. xxi.  
 ... MARSHALL and SPENCER.—Observations on the Cranial Nerves of *Scyllium*. *Quart. Journ. of Micros. Science*, vol. xxi.  
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DESCRIPTION OF PLATE X, illustrating Dr. Marshall's Paper on the Segmental Value of the Cranial Nerves.

*Alphabetical List of References.*

- a.r.* Anterior root of a spinal nerve.  
*B.* Buccal or mouth cleft.  
*Br.1.* First branchial cleft.  
*Br.2.* Second branchial cleft.  
*Br.3.* Third branchial cleft.  
*Br.4.* Fourth branchial cleft.  
*c.* Cerebellum.  
*g.* Ganglion on posterior root of spinal nerve.  
*m.b.* Mid-brain.  
*m.p.* Muscle plate.  
*n.* Notochord.  
*N.c.* Neural crest.  
*Olf.* Olfactory pit or cleft.  
*p.r.* Posterior root of a spinal nerve.  
*Sp.* Spiracular or hyomandibular cleft.  
*S.a.* Primary attachment of posterior root of spinal nerve.  
*S.β.* Secondary attachment of posterior root of spinal nerve.  
 2. Second head-cavity.  
 3. Third head-cavity.  
 I. Olfactory nerve.  
 III. Oculomotor nerve.  
 IV. Root of fourth or trochlear nerve.  
 V. Trigeminal nerve.  
     *V.b.* Maxillary branch of trigeminal nerve.  
 VI. Abducent nerve.  
 VII. Facial nerve.  
     *VII.d.* Buccal branch of facial nerve.  
     *VII.a.* Primary root of attachment of facial nerve.  
     *VII.β.* Secondary root of attachment of facial nerve.  
 VIII. Auditory nerve.  
 IX. Glosso-pharyngeal nerve.  
 X. Vagus nerve.

Figs. 1 to 4 illustrate the chief stages in the development of the spinal nerves, as shown by transverse sections through the spinal cord; they are copied, with some modifications, from figures given by Balfour in his paper on "The Development of the Spinal Nerves in Elasmobranch Fishes," published in vol. cxxxvi. of the *Philosophical Transactions*.

Fig. 1 shows the posterior roots arising as outgrowths from the neural crest on the summit of the spinal cord. Stage I.

Fig. 2 shows the shifting outwards of the primary attachment of the posterior root, caused by lateral growth of the mid-dorsal portion of the cord; also the first rudiment of the anterior spinal root on one side. Stage between I and K.

Fig. 3 shows the thinning of the primary root, *S.a.*, and the acquiring of the secondary root of attachment, *S.β.*, to the side of the cord some way below the primary root. In Balfour's figure this secondary attachment is not so clearly shown as in the figure given here. I have, from a direct investigation of the point, satisfied myself that this figure and the account given in the text represent correctly the process as actually occurring in Elasmobranchs.

Fig. 4 shows the definite acquirement by the posterior root of its secondary or permanent attachment, *S.β.*, the primary root having lost all connection with the cord, and being represented solely by the portion of nerve projecting above the secondary root. Shows also the anterior root more fully developed, and on the right connected with the posterior to form a complete spinal nerve. Stage K. This figure differs from Balfour's (plate xvii. I. 1) (1) in showing the posterior root on both sides instead of on the right side only; (2) in showing both anterior and posterior roots in the same section, to do which the section must be supposed to be not perfectly transverse, but somewhat oblique, as the anterior roots are not directly beneath the posterior, but a short way in front of them.

Figs. 5 to 7 illustrate the several stages in the development of the seventh and sixth cranial nerves of Elasmobranchs, as shown by a series of transverse sections through the mid-brain and roots of these two nerves at different ages. They are copied from figures of my own, illustrating a paper by Mr. Spencer and myself, on "The Cranial Nerves of Scyllium," *Quart. Journ. of Micros. Science*, July 1881.

Fig. 5 shows the primary root of attachment, *VIIa.*, of the seventh nerve to the top of the mid-brain. The stage figured is intermediate between the stages in the development of the spinal nerve represented in figs. 1 and 2. Stages between I and K.

Fig. 6 shows the acquirement of the secondary root of attachment, *VIIβ.*, to the side of the hind-brain, the primary root, *VIIa.*, still persisting: owing to the growth of the dorsal surface of the brain the primary roots of the two sides are now widely separate. Stage K.

Fig. 7 shows the persistence of both primary and secondary roots of attachment of the seventh, and the appearance of the sixth nerve at a position and in a manner identical with those of the anterior spinal roots. As in fig. 4, the section is a somewhat oblique one, as the sixth is not vertically below the seventh, but slightly posterior to it. Stage N.

Fig. 8. A diagrammatic view of the head of a young vertebrate embryo from the left side, showing the several segmental nerves and their relations to the visceral clefts. Only 4 branchial clefts have been represented; the hinder ones would be precisely similar to the others, and would, like the second, third, and fourth, be applied each by a branch of the vagus nerve.

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THE ANATOMY OF *MICROGALE LONGICAUDA*,  
WITH REMARKS ON THE HOMOLOGIES OF THE  
LONG FLEXORS OF THE TOES IN MAMMALIA.  
By G. E. DOBSON, M.A., M.B.

SCARCELY has the first part of my work on the Insectivora<sup>1</sup> been a month before the public when there turns up from that paradise of the Order, Madagascar, a little incongruous beast, evidently allied to the indigenous *Centetidae*, yet presenting certain special characters which, at first sight, would appear to render the formation of a new family necessary for its reception.<sup>2</sup>

A greater contrast, in external form at least, between allied species, than between this animal and the well-known tailless spiny Madagascar "ground-hogs" could scarcely be conceived; for besides its very small size, which is about that of *Sorex pygmaeus*, its fur is perfectly devoid of spines, and its quadrangular nearly naked tail is almost or quite double the length of the head and body. In fact, were it not for the shape and size of its ears, which are slightly longer than the muzzle, and resemble in structure those of the *Centetidae*, a skin of this animal without skull would very probably be mistaken for that of a very long-tailed species of *Sorex*. Nevertheless, as the following account of its general anatomy will show, *Microgale longicauda* must be relegated to the same family as the so-called "ground-hogs."

*Osteology and Dentition.*

The very small skull is destitute of ridges of any kind, and tapers evenly forwards, as in *Hemicentetes*, the characteristic absence of an interorbital constriction being as well marked here as in all known species of *Centetidae*. The zygomatic arch is also deficient. There are no traces of postorbital processes,

<sup>1</sup> *A Monograph of the Insectivora, Systematic and Anatomical.* By G. E. Dobson, M.A., M.B. London: John Van Voorst, January 1882. (Part I., including the Families *Erinaceidae*, *Centetidae*, *Solenodontidae*.)

<sup>2</sup> I understand that a diagnosis of this species under the above name has been read by Mr O. Thomas of the British Museum, at the last meeting of the Linnean Society.

and the lachrymal and infraorbital foramina are as in *Centetes*. The base of the skull shows similar structural agreement with that of the species of this family; the mesopterygoid fossa is narrow and deep, like that of *Hemicentetes*; the basi-splenoid sends down straight processes to complete the walls of the auditory chambers internally, but posteriorly they are formed by membrane only, while the tympanics are mere bony rings not forming bullae. There are no prominent mastoid or paroccipital processes. In the mandible the only important difference noticeable consists in the more transversely extended form of the condyle.

The dental formula is the same as that of *Hemicentetes* and *Oryzorictes*, and the teeth, on the whole, very closely resemble those of *O. hova*.<sup>1</sup> The three upper incisors are small and conical, very like those of immature specimens of *C. ecaudatus*; the first and second are nearly equal, and separated by a narrow interval; the first has a small posterior basal cusp; the second both anterior and posterior basal cusps; the third is close to the second, not half its size, acutely pointed, with a minute projection posteriorly at its base, and separated by a narrow space from the canine. The latter is the most vertically extended tooth in the upper jaw, and about one-third longer than the second incisor, which it closely resembles in form, and it has a single root. The first premolar is separated from the canine and the second premolar by a narrow interval; it arises from two roots, equals the canine in cross section, but in vertical extent only the third incisor, and has a short conical cusp with small anterior and posterior basal cusps; the second premolar is like the first, but longer and much larger in cross-section; it has a small internal basal projection; the third premolar is much longer, has a broadly V-shaped principal cusp, with one anterior and two postero-external small cusps, also a prominent internal basal process; the first and second molars have narrow V-shaped cusps, notched externally, and the third wants the postero-external angle of its V-shaped cusp, as in *Oryzorictes hova*. In the lower jaw the incisors, canines, and premolars (except the last) correspond closely in relative size and shape with those

<sup>1</sup> For the dentition and anatomy of this and of other species of *Centetidae*, see my *Monograph of the Insectivora* referred to above.

above, but their cusps are more obliquely directed forwards, and the second incisor and the canine have no anterior basal cusp. The molars are almost identical in shape with those of *O. hova*.

The clavicle is well developed; there are fifteen pairs of ribs; the scapula and pelvic bones resemble those of *Hemicentetes semi-spinosus*; the symphysis pubis is very narrow, and the tail is composed of about forty vertebrae. The humerus has a large supra-condylar foramen, which transmits the median nerve and a branch of the brachial artery; the ulna and radius are distinct, and there are five digits in the manus, which agree altogether with those of *Centetes*. The tibia and fibula are united about the commencement of the middle third of the leg, as in *Sorex*, thus differing from all the species of *Centetinae*. There are five digits in the pes, and the fifth is much longer than the hallux.

#### *Myology.*

The myology of the *Centetidae* having been described and illustrated in my work referred to in the foot-note to a preceding page, it will be sufficient to indicate here the chief points of resemblances or differences observed.

*Digastric* is simple, without trace of tendinous intersection or inscription, and not connected with the hyoid bone. *Mylo-hyoid*, *genio-hyoid*, *omo-hyoid*, and other muscles connected with the hyoid bone, as in *Centetes ecaudatus*. *Sterno-* and *cleido-mastoid* also as in *Centetes*, the former united for a considerable distance with its fellow of the opposite side, and forming a slender tendon in its anterior third, which unites with the margin of the *cleido-mastoid*.

*Subclavius* is well developed, arising from the sternum and articulation of the first rib, and inserted into the outer third of the clavicle, thus differing from all the species of the sub-family *Centetinae*, in which this muscle is absent.

*Scalenus anticus*, as shown by the position of the brachial plexus, is present.

*Pyramidales* are either very slender or absent, a few muscular fibres passing from either side of the symphysis pubis towards the linea alba may represent them. The other abdominal muscles are like those of *C. ecaudatus*, but the *obliquus externus*

is comparatively much less developed at its insertion into the symphysis pubis.

The muscles of the fore-limbs resemble those of *C. ecaudatus* very closely, with the following differences only:—The *biceps* is wholly inserted into the ulnar side of the *radius* near the insertion of the *brachialis anticus* into the radial side of the *ulna*. The tendons of the two muscles are not connected. The tendons of the *flexor sublimis* and *flexor profundus digitorum* are not joined by a tendinous slip at the carpus, and there is no adductor for the second digit.

In the hind limbs the *gracilis* does not unite with its fellow of the opposite side across the pubis, but is divided into two parts, an anterior from the ramus of the pubis and ilium, and a posterior from the ramus of the pubis and ischium; these parts are separated by the *adductor longus* and *brevis*, but are inserted one above the other into the prominent tibial ridge. These separated parts seem to point to an original continued condition of the muscle from the iliac ramus round the *symphysis pubis* to the ischiac ramus as we find it in all the *Centetinae*.

The *sartorius* is characteristically absent. The *adductor* mass appears to be divisible into two layers only, and the *adductor magnus* is not distinguishable, the nervous supply is wholly from the obturator nerve, and I can find no filament derived from the sciatic. *Biceps flexor cruris* has no connection with the femur, and its attachments in the leg are as in *C. ecaudatus*. *Semi-tendinosus* arises with it from the *tuber ischii*, and also by some fibres from the side of the tail; *semi-membranosus*, from the ramus of the ischium, is the largest muscle of the thigh. Both are inserted as in *Centetes*.

*Soleus* arises from the head of the fibula only by a slender tendon; *tibialis posticus*, from the inner side of the head of the rudimentary fibula and upper part of the interosseous ligament, forms a long slender tendon which passes round the malleolus with that of the *flexor digitorum longus*, and is inserted into the navicular bone. *Flexor hallucis longus* and *flexor digitorum longus* arise separately, the former from the head and shaft of the short fibula posteriorly as far as its junction with the tibia, and from the interosseous ligament below the origin of the *tibialis posticus*, the latter from the head of the tibia posteriorly,



and from the margin of the interosseous ligament; both muscles form long tendons which pass round the malleolus, and in the foot spread out, becoming superimposed, the *flexor digitorum longus*, as in all species of *Centetidae*, being superficial to the other, and sending tendons for the hallux and fifth toe, while those for the three middle toes are supplied by the *flexor hallucis longus*. (See *Monogr. Insectivora*, p. 82, pl. iv. fig. 11). Here, as in *Oryzorictes hova*, the union of the tendons of these two muscles is much more complete than in the *Centetinae*, nevertheless the tendon for the hallux can be distinctly seen to be given off by the *flexor digitorum longus*. A large *flexor accessorius* is inserted into the outer side of the conjoined tendons. The other pedal muscles are precisely as in *C. caudatus* and *O. hova*.

Although the leg bones are united so high up, there are here, as we see, the very same muscles, having the same relations as in the sub-family *Centetinae* (all the species of which have separate tibiae and fibulae), a condition very indicative of close affinity, for in *Solenodon cubanus* of the allied family *Solenodontidae*, although the fibula and tibia are separate as in the *Centetinae*, the tendon of the *flexor digitorum longus* is very slender, and inserted into the side of the tendon of the *flexor hallucis longus* which supplies all the toes.<sup>1</sup>

### Visceral Anatomy.

In the form and arrangement of the lingual papillae, and in the whole structure of the intestinal canal and of the liver, the closest resemblance to *Centetes caudatus* is apparent, although

<sup>1</sup> In *Potamogale velox*, where the leg-bones are united as in this species, the tendon of the *flexor digitorum longus* is reduced to a mere thread, inserted as in *Solenodon cubanus* into the side of the tendon of the *flexor hallucis longus*; were this rudimentary tendon to disappear altogether, we should then have a condition similar to that in *Sorex*, where a single tendon, arising from a single muscle, passes into the foot and supplies all the toes. Hence it is evident that the single tendon in *Sorex*, and probably in all other genera of mammalia having but a single long flexor of the toes, is the homologue of the *flexor hallucis longus*, and not of the *flexor digitorum longus*, as it appears to be considered from having that name applied to it. It would therefore seem advisable, in the interests of muscular homology, to change these misleading names for such terms as *flexor digitorum fibularis* (vel *externus*) and *flexor digitorum tibialis* (vel *internus*) respectively.

the stomach has the pyloric and œsophageal apertures much more widely separated; the form of the epiglottis and of the larynx and trachea are also quite as in that externally very differently shaped species, the only difference observable being found in the fewer number (26) of the tracheal rings. The right lung is divided into three lobes, the left is undivided; the azygos lobe is quite similar to that of *Ericulus setosus*, and the heart and great vessels are formed on the same plan as in that species.

As in all the *Centetinae*, the male and female generative organs open within the same fold of integument which forms a shallow cloaca. The penis, like that of the other species, is capable of being wholly retracted within this cloaca, and lies strongly bent and folded upon itself against the symphysis pubis beneath the integument. It has a well-developed cartilaginous bone, and is provided with strong *erectores* and *levatores* muscles comparatively much larger than in *Centetes*. In the form and position of the testes, however, the chief departure from the general type is observable; each testis, instead of being placed close to the kidney as in the *Centetinae*, lies in the pelvis; its epididymis is remarkably large, especially the globus minor which is attached by a short ligament to the inferior surface of the membranous portion of the urethra near the neck of the bladder. The vas deferens is at first tortuous, but passes directly between the prostate and the bladder, accompanied by the duct of a long narrow gland-like organ<sup>1</sup> connected loosely by a fold of peritoneum with the testicle, and getting beneath the sheath formed by the large *acceleratores urinæ* muscles, enters the cul-de-sac at the commencement of the penial urethra.

The female organs, on the other hand, correspond very closely with those of the *Centetinae*; the uterus and ovaries are almost perfectly similar to those of *C. ecaudatus*, but the vagina is very capacious and well defined by a sudden constriction from the uterus, which has a short corpus uteri. I cannot detect, however,

<sup>1</sup> This, on microscopic examination, is seen to be composed of a long convoluted tube filled with reddish granules, and appears to represent the *vesiculæ seminales* in their reduced condition.

transverse processes in either, which are so well marked in other species of that sub-family.

A review of the above-described characters presented by this very interesting species show that it must evidently be classed with the *Centetidae*, forming with *Oryzorictes hova*, a special sub-family, for which I have already proposed the name *Oryzorictinae*.

THE CURLED HAIR AND CURVED HAIR FOLLICLE  
OF THE NEGRO. By T. P. ANDERSON STUART, M.B.,  
*Assistant to the Professor of the Institutes of Medicine,  
Edinburgh.*

Mr. CHARLES STEWART, in a paper read before the Royal Microscopical Society, January 1, 1873, compares the Negro with the European scalp, and states that in the Negro "the portion of the hair and follicle imbedded in the skin is much longer, and is also remarkably curved, so that it commonly describes a half circle. The papilla at the base of the follicle consequently either lies horizontally, or even becomes directed obliquely inwards towards the subjacent bone. In other respects there is no great difference, but perhaps the sebaceous gland is somewhat smaller." He thus records the fact of the curvature, but draws no conclusion from it, and, so far as I am aware, his observation has not yet been confirmed.

I have recently had an opportunity of verifying this statement, to a certain extent, from sections which I have made from a scalp for which I am indebted to my colleague Dr. Arthur Thomson of the University Anatomy Rooms. There was some difficulty in obtaining a good profile view of the follicles, since, though follicles were abundantly present, they were nearly all in transverse or oblique section. I now perceive that this is actually due to the curve of the follicle, for if the knife does not cut the follicle exactly in the plane of its curve, necessarily we do not see the whole length of it. But those that are cut in that plane show a most graceful curve, and show also that the pigment of the rete mucosum extends down the follicle only so far as the opening of the sebaceous gland. In the case examined by me the curve certainly is not commonly a half circle. Camera lucida drawings prove that it is almost invariably about a quarter of a circle.

In his drawing, Stewart puts the papilla nearly vertically below the opening of the follicle, so that the complete circle would lie nearly quite within the scalp. In my specimens the

follicles are all segments of a larger circle which, when complete, is half or more without the scalp, so that the papilla lies very much laterally to the opening of the follicle. I believe that these differences in our descriptions are not without importance, as will presently be seen.

It seems to me that this curve of the hair within the follicle persisting in the hair without the follicle, accounts for the curl of the hair. If that be so, then, if the curve of the follicle were a half circle placed as Stewart describes it, the hair would tend to grow into the scalp, or the curled hair would be forced to twist itself laterally, and to lie close to, and flat on, the surface of the scalp. But according to my description the hair would grow freely up out of the scalp, and the plane of the curl would be vertical to the surface of the scalp, and this is what we see in nature. The hair muscle seems invariably to be placed in the concavity of the curve, and between it and the follicle is the sebaceous gland. Both muscle and gland are extremely well developed in my specimens.

SOME OF THE PATHOLOGICAL CONDITIONS IN THE  
MEDULLA OBLONGATA, &c. IN A CASE OF LOCOMOTOR  
ATAXIA (*TABES DORSALIS*). By G. SIMS  
WOODHEAD, M.D., M.R.C.P.E., *Demonstrator of Practical  
Pathology in the University of Edinburgh.* (PLATE XI.)<sup>1</sup>

It was pointed out some time ago by Dr. D. J. Hamilton,<sup>2</sup> that in acute myelitis, produced artificially, the axis cylinders in the columns of the cord become immensely enlarged at certain points. At intervals dilatations or oval tumours in the longitudinal course of the nerve appear. Between these dilatations the axis cylinder is constricted, this constriction becoming more and more marked until at length the swellings are quite separated. In some of the swellings we have concentric rings. These swollen masses divide rapidly, and stain deeply with carmine, though at first they appear to be perfectly homogeneous. After dividing, the rounded masses make their way through the nerve sheath, and involve the surrounding tissue, where they are evidently identical with the colloid bodies found in chronic diseases of the cord. If the inflammation be more severe the masses become transparent and granular, and several nuclei are formed in their interior mother cells (which breaking down give rise to suppuration). He also describes changes in the nerve-cells and neuroglia, which, as we shall see, closely resemble those to be described as present in at least one case of *tabes dorsalis*.

(1) Fissiparous division of the nuclei of the connective tissue corpuscles; (2) Changes in blood-vessels common to all inflammatory conditions.

He then suggests that the absence of axis cylinders and the presence of colloid bodies in locomotor ataxia, epilepsy, and syphilitic medulla, may all be due to this condition of inflammation of the cord in which the axis cylinder is becoming varicose, and there is distension of the nerve sheath.

Professor Stricker, in his *Vorlesungen* for 1880, points out that he did not believe in these changes at the time the experi-

<sup>1</sup> Presented as a Thesis for graduation as M.D. in the University of Edinburgh, and to which a Gold Medal was awarded.

<sup>2</sup> *Microscopical Journal*, 1875, p. 338.

ments were made, but that, since he has seen some similar preparations made by Dr. Weiss of Vienna, he has become thoroughly convinced of the accuracy of Dr. Hamilton's observations.

In 1864 Frommann<sup>1</sup> described in a case of sub-acute myelomeningitis a tumefaction of the axis cylinders of the nerves of the white matter. This he noted both at, and for some distance beyond the lesion.

In 1871 Charcot<sup>2</sup> examined three cases in which after inflammation of the cord there was tumefaction, not only of the axis cylinders, but also of the cells of the anterior horns.

Case 1.—Cord divided by a bullet. At point of division, nothing but increased number of myelocytes, many of which are enlarged and surrounded by a mass of protoplasm. (Masses similar to those described above?).

At certain points in the cord (near traumatic lesion) the axis cylinders are seen to have dilatations and constrictions similar to those artificially produced. Nerve cells swollen and more clearly marked than usual.

In Case 2 reported by M. Joffroy to M. Charcot, there is total destruction of the cord at one level, above and below which may be made out all the above changes.

Whilst in Case 3.—(Sub-acute inflammation),—he noted—

(1) The capillaries present, here and there, moniliform dilatations, and in their walls are numerous nuclei, with here and there masses of fatty granules.

(2) The axis cylinders, stripped of myeline, are thicker than normal.

(3) Numerous myelocytes, both free and enveloped in protoplasm.

(4) Connective tissue reticulum, in which the meshes of the network are notably thickened. In the posterior columns of the cord, the axis cylinders have become enormously thickened, and are here and there surrounded by only a small amount of

<sup>1</sup> "*Untersuch.: über die normale und Patholog. Anatomie des Rückenmarks.*" 1864, pp. 98-105.

<sup>2</sup> "*Sur la tuméfaction des cellules nerveuses motrices et des cylindres d'axe des tubes nerveuses dans certains cas de myélite.*"—*Arch. de Physiol.* 1871-72, pp. 93-100.

myeline. Between the tumified axis cylinders the meshes of the reticulum are thicker than normal. Cells in anterior horn are swollen.

On longitudinal section, these axis cylinders are seen to be moniliform.

Charcot considers, then, that these are the changes which take place during the course of acute or sub-acute myelitis, and he quotes a case of Dr. Müller's, in which much the same appearances were noted, not only in the region of a softening of the brachial enlargement, but also in the lateral columns which were affected by a consecutive descending myelitis; and he goes on to point out that those changes in the axis cylinder may take place before the increase of connective tissue, in the chronic condition, but it undoubtedly does occur in the acute stage.

These changes in the axis cylinder, moniliform swelling and constriction, in fact, may be regarded as being one of the most marked of the anatomical expressions of a myelitis of the cord, and they may be so prominent as to obscure other histological changes. The process, if not checked, will go on to complete disintegration of the axis cylinders of the cord.

M. Hayem<sup>1</sup> describes very much the same conditions. In addition to which he describes a gelatinous material, which in many respects resembles an exudation I have noticed under the desquamating epithelium of the 4th ventricle in a case of epilepsy.

In the case before us, in connection with which I have made the following short abstract of the *post-mortem* report, kindly supplied to me by Dr. D. J. Hamilton, Pathologist to the Royal Edinburgh Infirmary, to whom I am deeply indebted for valuable assistance and suggestions during the time that I was working in the University Pathological laboratory. I may here also express my thanks to Professor Grainger Stewart for allowing me to publish in this thesis the results of my observations on his case. A number of most interesting facts have been observed, some corroborative of the above observations, and others bearing further in no small degree on the nature of the disease.

<sup>1</sup> "Note sur deux cas de myélite aiguë centrale diffuse."—*Arch. de Physiol.* 1874.



—, aged forty-nine, foreman engineer, died 10th September 1878, under the charge of Professor Grainger Stewart.

*Post-mortem* examination 11th September 1878. History was that of locomotor ataxia lasting for twelve years, and of a most marked character. *Post-mortem* rigidity very slight; no trace of bed sores. Body much emaciated. Pupils smaller than usually met with in the dead subject. Cheeks sunken; no glossiness of the skin in any part. All the muscles of both forearms greatly wasted; those of the right more than those of the left. Legs equally emaciated, most markedly so in the calves. Fibrous adhesions of pericardial surfaces in front. Fibrous pleural adhesions over both lungs, in which too are caseous catarrhal pneumonic nodules, softening, &c. Cavities in upper lobes of both lungs. Liver very fatty. Small gall stones in gall bladder. Kidneys; on each side, pelvis distended, atrophy of substance from pressure. Deposits of urinary salts in this atrophied substance. Ureter dilated and thickened, and on left side completely closed by a sigmoid twist in the ureter fixed by a band of cicatricial fibrous tissue. In the pelvis was an abscess containing foetid green coloured pus, situated between the rectum and the bladder. Bladder contained a small quantity of purulent urine, and was in a chronic inflammatory condition, thickened, congested, and of an ashen grey colour on the surface. Stomach deeply bile stained and contained a quantity of bile-stained fluid; the small and large intestines exhibited nothing worthy of note.

*Head*.—Frontal bone peculiarly thick, in front measuring  $\frac{5}{8}$ ths of an inch (Dipl e hard): in occipital region  $\frac{3}{8}$ ths of an inch, and in temporal region  $\frac{3}{8}$ ths of an inch. Dura mater apparently normal, but somewhat adherent to the calvarium. A considerable amount of fluid at the base of the brain. Organ weighed almost 3 lbs. Vessels at the base (vertebral and basilar arteries) considerably congested. At the tip of the temporo-sphenoidal lobe on either side the arachnoid and pia mater are much thickened, and apparently firmly adherent to the brain substance. The vessels throughout the brain substance seemed normal, with the exception of slight thickening of the coats of the larger branches. No change was observable with the naked eye in any of the cranial nerves. The choroid plexus and its continuation were of a brown colour.

*Spinal cord*.—A considerable amount of cerebro-spinal serous fluid. About the level of the 7th cervical vertebra the dura mater was considerably thickened and adherent to the bodies of the vertebr e as high as the third cervical vertebra. On section of the organ it seemed very much wasted in the dorsal region, more especially at its lower part. The whole of this region showed the most marked grey degeneration in the posterior columns, extending apparently throughout their entire extent. The grey gelatinous substance was not soft, but peculiarly firm in texture. It could be traced downwards throughout the lumbar region, and upwards to the medulla oblongata, where it was apparently lost. In the cervical region it seemed more limited to the postero median column.

*Note.*—We shall see later, on microscopic examination, that this limitation is only apparent, as the postero external columns are slightly affected even in the cervical region, in addition, to which the direct cerebellar tract is also involved in the degenerative changes.

After hardening the cord, medulla oblongata and certain of the nerves, these were subjected to a careful examination, with the following results. (It will be well in giving these results first to describe the changes as seen in a single nerve tube, and then the changes taken collectively.)

Fig. 1 is a drawing made from a longitudinal section taken from the direct cerebellar tract at the level of the 1st cervical vertebra, in which (*a*) the axis cylinder, still a homogeneous mass without the slightest appearance of segmentation, is swollen at certain points, constriction being as yet but imperfectly marked. (*b*) The moniliform appearance of the axis cylinder is here seen, but as yet no traces of segmentation; at (*c*) these homogeneous masses are breaking down, and we have the formation of a mother cell (swollen axis cylinder) containing smaller masses (broken down axis cylinder substance) corresponding to the colloid bodies seen in chronic myelitis. At (*d*) this condition is extremely well marked, in addition to which we here note the results of a further degenerative change, as in the position formerly held by the axis cylinder we find an enormous number of small fatty globules and granules which stain black with osmic acid. From a section of the same cord at the same level, but from the postero external column (funiculus cuneatus), the five drawings (figs. 2 to 6) are made to illustrate these changes in the axis cylinder as seen in transverse section of the nerve, and in four drawings (figs. 7 to 10) the changes as seen in longitudinal section. In fig. 7 the irregular constrictions appear at intervals along the axis cylinder, the myeline remaining intact or but slightly affected. In fig. 8 the alternate constriction and swelling has become much more pronounced, and the varicosity is now well marked.

Fig. 9. The swollen homogeneous mass or collection of axis cylinder substance appears to have become vacuolated or nucleated, and we have now the colloid body stage above mentioned. Were this an acute myelitis, it seems highly probable, from the experiments and cases quoted above, that we should have the

purulent stage following this, and the axis cylinder breaking down to form pus corpuscles; whilst, on the other hand, had we to deal with an extremely chronic condition, this process would continue no further, and there would remain a mass of colloid bodies, such as were found in other portions of this cord, where the process was less acute than it was at the point we are examining. Here we have to deal with a subacute process, or at all events a not very chronic condition, as a result of which the colloid bodies undergo the further change noted above as a fatty degenerative change (fig. 10).

Fig. 11 shows the same changes under a much lower power.

Fig. 12 is a drawing made from a section through the valve of Vieussens at the point of decussation of the fourth nerve, in which the colloid degeneration following breaking up of the axis cylinder may be observed. It will be seen that very few of the nerves remain unaffected in this region.

In fig. 13 may be seen the great increase in the amount of connective tissue, bands of which separate, by considerable intervals, the masses of breaking-down axis cylinders, or colloid bodies derived from the swollen axis cylinders. If this drawing be compared with a section of the healthy direct cerebellar tract at the same level, it will be seen how great is this increase. In the healthy condition we have the regular arrangement of the nerve fibres (seen in transverse section), which are closely packed together, with just sufficient connective tissue to support them and their blood-vessels, whilst in the pathological condition we have dense masses of this connective tissue taking on carmine staining readily, and, embedded in this, the granular masses so often referred to. All these changes, from the first condition of moniliform axis cylinders, said by Charcot to be characteristic of acute inflammations of the spinal cord down to the colloid bodies, held to be almost pathognomonic of chronic myelites, recognised as such, along with the interstitial changes and the changes in the vascular supply, point to a condition of myelitis, and, so far, we have found these changes in certain definite parts of the central nervous system that we have examined in this case of locomotor ataxia.

We then find in all the sections we have examined (some 700 in number) of the central, and some other parts of the nervous

system, that here at least there is a degeneration of the axis cylinders, which commences as a swelling at certain points along the course of the nerve. We have thus moniliform cords formed within the medullary substance. In these moniliform cords a colloid change goes on (the colloid bodies staining pretty deeply with logwood and carmine, but not characteristically with iodine solution); whilst, as the disease becomes more advanced, the medullary substance becomes decreased in amount, and granular masses are formed by the fatty degeneration of the colloid bodies. Accompanying these changes, even in the earliest stages (especially in the direct cerebellar tracts), there is, as we have seen, a considerable increase of connective tissue.

In the posterior columns of the cord, where the disease commenced and is now in a more advanced stage, the axis cylinders have disappeared altogether, and the medullary sheath, if present, cannot be distinguished from the surrounding interstitial tissue. In the direct cerebellar tract we have the clear globular masses (swollen axis cylinders seen in transverse section) surrounded at first by the medullary sheath, in which is to be seen distinct lamination. Wherever these masses are in great numbers the axis cylinders, as such, have disappeared.

The vessels and the perivascular spaces are seen to be loaded with fat, whilst to the surrounding structures have been carried numerous white blood corpuscles which, along with the proliferating connective tissue cells, form a dense connective or interstitial tissue.

The process, then, is one of colloid and then fatty degeneration, absorption of fat, which is carried off partly by the perivascular lymph channels, exudation of white blood corpuscles, proliferation of connective tissue corpuscles, and complete replacement of the nerve fibres by connective tissue. This loading of the connective tissue with wandering cells is seen also in the pia mater, in which position, too, the perivascular spaces are filled with fatty particles, and in some instances we have a substance giving a black reaction with osmic acid in the vessel itself.

Without quoting the various authorities for and against the following theory, it will be well to give the facts on which the supposition that we have to deal with an inflammatory con-

dition, commencing first in the nerve fibre (probably in the axis cylinder), and secondly, involving changes in the surrounding structures, is based.

1. In this myelitis the swelling of the axis cylinder may be the chief or only abnormal condition to be made out.

2. Up to the very margin of the diseased area the surrounding fibres are perfectly healthy (fig. 13).

It appears to be, then, a disease of the continuation of a process of a nerve cell in the posterior spinal ganglion, or a disease of the nerve fibre continued direct from the periphery. The changes follow an ascending course, and only those cells and nerves are affected secondarily with which the primarily diseased nerve or nerve-cell is in direct communication.

That the connective tissue growth is secondary appears to be evidenced by the fact that its area of increased growth is so definitely localised, and it appears to be due to the decreased pressure owing to the absorption of the axis cylinder. The medullary sheath, if persisting, has pressure brought to bear on one side only, and in addition to this there is the factor of secondary irritation of degenerating tissues to be brought in at this point. Then, too, if the increase in the connective tissue were a primary change, we should expect that growth would take place regularly, and pretty equally, between the several nerve tubes and the patches of granular axis cylinders, when present, would appear as minute carmine or osmic acid stained points; whereas we have, instead of this, only a few of the stained patches, and these are of considerably greater size than could be accounted for by a single axis cylinder, or even several, and between these patches the bands of newly-formed connective tissue are of considerable extent, but irregular in both size and distribution. The change, then, may be described as a trophic disturbance in the axis cylinder, which, in this instance, must be regarded as the nucleus of a cell. It is an inflammatory change, followed by a degenerative process. There is a breaking-up of the axis cylinder of which we have a gradual atrophy. The pressure is thus taken from the interior of the medullary sheath, which contracts just as in the case of a vessel from which the centrifugal pressure has been removed in endarteritis obliterans in a healing wound, where the place of the active

blood pressure is taken by the passive clot. The connective tissue has no internal pressure to contend against, and it increases in quantity. It takes the place of the obliterated tubes, and so comes more prominently forward, and we have the hard, grey, sclerosed mass so characteristic of *tabes dorsalis*. Examining this from a developmental point of view, we have the fact that the central nervous system is developed from the epiblast, in so far as the nerves and cells are concerned, but it must be remembered that in all probability the connective tissue is primarily derived from the mesoblastic layer.

The processes of nerve cells (epiblastic), according to Flechsig,<sup>1</sup> grow in the direction of least resistance, but whichever way they take they have to overcome a certain resistance as they pass into the mesoblastic layer. So long as these cell processes are healthy and vigorous they hold their own, and we have a balance of power. As soon, however, as atrophic changes commence in the epiblastic processes, the mesoblastic elements are no longer resisted to the same extent, and the sheaths of the nerve fibres are pressed together as soon as the axis cylinder has left its place, from which we should argue that the connective tissue plays an active, but, at the same time, quite a secondary part in the process. This can scarcely be insisted upon too strongly, as it bears no unimportant relation to the causation of locomotor ataxia, although I am perfectly aware that in the above statement there are many points in which there is wide divergence from what Leyden, Charcot, and Lange have laid down. Leyden,<sup>2</sup> in relation to this, holds that in locomotor ataxia there are no inflammatory changes either in the nerve elements or in the connective tissue, but a simple atrophy of the nerve fibres. Charcot and Friedrich both contend that there is a myelitis, but ignore to a great extent the primary lesion in the nerve, and lay far too great stress upon the formation of connective tissue, making it the primary, rather than the secondary process.

Then Lange<sup>3</sup> lays it down as his opinion that the great factor in locomotor ataxia is a secondary degeneration due to primary

<sup>1</sup> Flechsig, P., *Die Leitungsbahnen im Gehirn und Rückenmark*.

<sup>2</sup> Leyden, E., *Klinik der Rückenmark's Krankheiten*, 1874.

<sup>3</sup> Quoted from Erb on "*Tabes Dorsalis*," in Von Ziemssen's *Cyclopædia of the Practice of Medicine* (English translation).

affection of the posterior nerve roots, caused by pressure of thickened and inflamed meninges. It may be a secondary degeneration, but it is probably of an inflammatory nature.

Dr. Weiss of Vienna has lately advanced a theory, which is much more in accordance with the above, that the fibrous-looking connective tissue is derived from the elements of the nerve fibre. It will thus be seen that, with the exception of Dr. Weiss, all the above writers, as we have said, consider the connective tissue new formation the primary process in the disease. From evidence afforded by the preparations under consideration, this, I think, is proved not to be the case.

That this is the only form of pathological change in cases of locomotor ataxia, I am not for a moment prepared to assert; in fact, there seem to be several lesions in the cord which may give rise to very similar symptoms. For instance Erb,<sup>1</sup> describes a condition very dissimilar from the above. In the first stage he describes a thickening of the interstitial tissue and an increased development of Deiter's cells (Neuroglia cells). This, in all probability, is a secondary process. He then goes on to state that in the nerve fibres, apart from the simple emaciation, dwindling, and final disappearance, there are no changes. A slight thickening of the nerve fibres is rare; there is usually no breaking down of the medullary sheaths, nor fatty degeneration of the same, nor swelling of the axis cylinder. It is merely a question of simple atrophy and disappearance of the fibres. "Granular cells are found," says he, "the vessels are thickened, the walls are rich in nuclei, corpora amylacea are scattered throughout the tissue in greater or less numbers." The later stages he describes pretty much as we have done. From the conditions given I am undecided as to what the change really is, but I am strongly inclined to believe that even here we have to do with a primary nerve change, of which the first indications have been overlooked. The corpora amylacea point to this fact, and those who have studied this subject know how easy it is to overlook the primary stages of varicosity of the axis cylinder, when only transverse sections of the cord are examined.

*Distribution of affected areas.*—As I could only examine the

<sup>1</sup> *Ibid.*

medulla oblongata and the nerves passing to it, I shall here simply give the general distribution of the anatomical changes in a typical case of locomotor ataxia, after which we can consider the distribution of the morbid changes in the medulla oblongata, &c., in the case before us.

It has been pointed out by M. Pierret<sup>1</sup> that the essential region in which changes must take place is the postero-external column, and he gives a number of cases in which this is exemplified. Then, too, he affirms that the postero median column (*funiculus gracilis*), along with the direct cerebellar tracts, may both be in the last stages of degeneration, with scarcely a symptom of locomotor ataxia present. In cases of Pott's curvature of the spine, for instance, where we have ascending degeneration in these tracts following myelitis due to pressure of the vertebræ. It will be well to omit the discussion of the statements regarding the passage upwards of the posterior nerve root fibres to the postero-external column and the posterior median column (*fasciculus gracilis*). We will simply note that in the case before us the posterior external columns are in an advanced condition of sclerosis, and that this extends upwards, especially in the postero median column. The posterior nerve roots have become mere threads, the cells in the posterior horn of grey matter are becoming fattily degenerated or pigmented; whilst a few cells in the anterior horn are also shrivelled in appearance, and some seem to be undergoing a fatty change. (These changes will probably be made the subject of a special paper in connection with the case, which I am not able to give here.)

In addition to these changes, we have in the upper cervical region an advanced stage of degeneration of the direct cerebellar tract, which, as pointed out by Flechsig, Pierret, Gowers, Duval, &c., is as a rule seen only in ascending degeneration of the cord.

The point in the posterior external column, according to Pierret, at which the change commences is between the arching commissural fibres of Todd and Vulpian, and the deeper set of fibres of Dean and Kölliker, which come from the posterior roots; or the sclerosis, he thinks, may commence in one or other of these sets

<sup>1</sup> Pierret, A., "Note sur un cas de Sclérose primitive, du faisceau median des cordons postérieurs," *Archives de Physiologie*, 1873, pp. 74-79.



of fibres. In one case he describes it (*loc. cit.*) as commencing as two small islets of sclerosis in the external bands of the posterior column, occupying the inner border of the posterior horns, in which are both commissural and internal radicular fibres, though he cannot say which set of fibres was first affected. This patch tends to spread inwards to the postero median column, and externally to the posterior horn. With the affection of the postero median column no new symptoms are developed, though this is not the case when the external band is implicated.

The internal root is next affected, after which the posterior horn and the direct radicular fasciculi. Then the external radicular fibres and the direct cerebellar tracts are involved, the train of symptoms in all cases keeping pace with the extension of the diseased process, and occurring in as regular order as does this extension of the disease.

These symptoms are at first formication, numbness, slight clumsiness, and lightning pains, followed later by sensory troubles, hyperæsthesia, anæsthesia, and motor inco-ordination. Then follow cramps and subsultus in the limbs, crossed paresis, which ends in almost complete paralysis, often with rigidity. We have then to do with a fixed morbid change, accompanied by symptoms which occur in regular sequence from below upwards. For instance, as Pierret points out, we may find anæsthesia in the legs, whilst in the arms and hands there may be only lightning pains, and so on throughout the whole series of symptoms.

In the case before us, as in Pierret's cases, the ascending degeneration in the postero median column stops at the nucleus of this column in the restiform body—just as we should expect from a consideration of Flechsig's observations. That in the posterior external zones and the direct cerebellar tracts is, however, continued into the cerebellum by the inferior peduncle. In a section through the inferior cerebellar peduncle the morbid process is very well marked, as it is also in the fibres running between this tract and the auditory nerve.

*Changes in the medulla oblongata and in the nerves passing to it.*—The changes in the axis cylinder have from time to time been noted in both the grey and the white matter of the brain ;

and in Virchow's *Archiv* for 1873, Bd. 58, is a resumé of what was known on the subject at that date.<sup>1</sup>

It is here pointed out that this change occurs in Bright's disease, in which case it is ascribed to a degeneration of the nerve fibre.

Hadlich<sup>2</sup> also describes a varicose condition of the axis cylinder of the processes of the ganglion cells of the grey matter of the cerebellum.

Grobe noted the same changes in the brain of a rabbit in both white and grey matter, and he records cases where the condition was noted in the brains of patients under his charge, for further details of which see Virchow's *Archiv* for 1873. In the optic nerve Zenker<sup>3</sup> and Virchow<sup>4</sup> describe these varicosities (swellings of the axis cylinders) in the retina, and that they are of frequent occurrence is evidenced by the fact that they were again described by H. Müller, Schweiger, and Nagel in *Die Archiv f. Ophthal.* Bd. iv. and vi. All these authorities concur in noting these moniliform axis cylinders in the retina, but they do not seem to have traced the change into the optic nerve. I have, however, managed to demonstrate these changes in the first three quarters of an inch of the nerve after it has left the retina, in the decussation of the nerves, and for a short distance beyond this in the optic tract; and as the change occurs in every part of the nerve that I had to examine, it appears to be even more than probable that the presence of this change will be before long demonstrated in the whole extent of the optic tract as far as the corpora quadrigemina, in which case we should have a completion of the chain of disease from the posterior root zones on the one hand, to the optic discs on the other. In the meantime, it is to be remembered that these changes do occur in the optic tract, and that they are identical with those earlier changes we have mentioned as seen in the posterior columns of the cord. Though here the changes are never so far advanced as we have found them in the cord, we have, in fact, the moniliform appearance and the coloured masses,

<sup>1</sup> Prof. M. Roth, *Ueber varicöse Hypertrophie der Nervenfasern des Gehirns*, p. 255; Dr. Otto Obermeier, *Varicose Axencylinder im Centralnervensystem*, p. 323.

<sup>2</sup> Virchow's *Archiv.*, Bd. xlv.

<sup>3</sup> *Archiv. f. Ophthal.* Bd. ii.

<sup>4</sup> Virchow's *Archiv.*, Bd. x.

smaller, it is true, in this region, as the fibres are smaller, but even more distinctly marked than in the cord. Erb,<sup>1</sup> writing on the disturbances of the auditory sense, remarks that observations have been made by various authors as to these disturbances of hearing. These are considered by Lucae to be merely accidental, where he demonstrated this to be the condition in two cases that came under his observation. Erb, however, mentions one case which he thinks proves that tabes may be also accompanied by purely or principally nervous deafness, which he considers depends in all probability on atrophy of the auditory nerve, and is thus analogous to atrophy of the optic nerve.

After quoting the case, in which there was no *post-mortem* examination, he goes on to say:—"But, at all events, the question of the occurrence of affections of the auditory nerve in tabes requires more accurate investigation." In the case before us we have undoubtedly an affection of the auditory nerve, which, along with the clinical features of the case (when published), will go far to set aside any doubts as to the condition of the auditory nerve in some cases of locomotor ataxia.

The nerves are in a similar condition to those of the optic nerve and spinal cord. There are numerous colloid bodies along the whole external course of the nerve, and for some distance into the medulla oblongata. In addition to these changes, the swollen axis cylinders are present in great numbers. The varicosities on these are remarkably well seen, and the whole nerve presents the typical neuritic (myelitic) condition, as it was described earlier. As yet, I have not been able to trace this change to the nerve nucleus, but it appears that the change has been followed quite far enough to enable us to account for auditory disturbances.

In the fifth nerve the change is in some places peculiarly well marked. We shall consider this later.

The fourth nerve, examined at its periphery, remains unchanged; but at the valve of Vieussens, at the upper part of the fourth ventricle, at which point the fourth nerves decussate and come into close contact with the deep band of Reil (forming the roof of the fourth ventricle or valve of Vieussens), we have a number of these colloid-looking bodies (fig. 12) in the various forms;—

<sup>1</sup> Erb, *loc. cit.* p. 581.

the varicose axis cylinders and the fatty debris of some of these broken down cylinders. In this position the axis cylinders must be those of one of two sets of fibres, or of both,—either of the posterior fibres of the decussating deep bands of Reil, or of the decussating fibres of the fourth nerve. If the band of Reil be affected, these masses, by the pressure and the irritation they set up, are likely to affect the fourth nerve, first irritating it, and then causing a certain amount of loss of function and paresis. We have, in fact, pretty much the same set of conditions as we shall describe later in connection with the descending root of the fifth nerve, as it caps the posterior horn of the grey matter in the upper cervical region of the cord.

The changes, if in the fourth nerve itself, would readily account for symptoms, but the former appears to be the real state of affairs. The other motor nerves appear to be unaffected, and, so far, I have been unable to demonstrate any typical change in the olfactory nerves.<sup>1</sup>

These sensory nerves then, with the above exception, are affected by a change which corresponds in its histological appearances to that found in the posterior or centripetal spinal tracts, whilst the motor nerves appear to be quite unaffected, unless it be at some point where they come into close contact with some band or other connected with the sensory area.

From these facts we are compelled to come to the conclusion that we have an affection of nerves which are in no way connected externally with the spinal cord, which do not run directly into the root zones, which in all probability degenerate from the periphery towards the centre, and which, to all appearances, present very much the characters observed in parenchymatous neuritis, as described by M. Joffroy.<sup>2</sup> He points out that under certain conditions, such as occur in lead poisoning, in infectious diseases, rheumatic diathesis, &c., we have changes in the nerves (confined to certain bands, however), which can best be described as a parenchymatous neuritis. It appears to be, as he describes it,

<sup>1</sup> In some sections of the olfactory nerve a number of colloid bodies were to be made out, and some of the delicate nerve fibres appeared to have their axis cylinders slightly moniliform, but the change was not nearly so definitely marked as in the other sensory nerves.

<sup>2</sup> Dr. Alex. Joffroy, "De la Névrite Parenchymateuse spontanée Généralisée ou Partielle," *Arch. de Physiologie*, 1879, p. 178, &c.

identical with the change we have under observation in the case now under consideration.

In his observations he gives cases in which the symptoms were extreme pain, darting like those lancinating pains of locomotor ataxia. At the same time there was wasting of muscles in certain regions, affecting the positions of the limbs. Reaction of degeneration in affected muscles. The first case was a convalescent from small-pox, who died of tuberculosis. Here the nerves were found to have lost the axis cylinder and the myeline. The pain commenced before the atrophy, and in all probability a myositis followed a neuritis, the reverse seldom occurring. In these cases examined by M. Joffroy, however, the brain and spinal cord were perfectly healthy. He then gives a series of cases in which there is a general parenchymatous neuritis. In one case mentioned he states that notion of position is quite lost when the eyes are closed; skin reflex slightly affected; no eye troubles; no lancinating pains or gastric crises; no paralysis. There comes on rapid affection of muscles (extensors), with a great amount of inco-ordination (pointing to the fact that the fibres when in the cord have only the same function that they have in the peripheral nerves). There comes to be great atrophy of the muscles, along with paralysis, especially in the lower limbs, whilst the electric contractility is lost or greatly diminished. Patient died of phthisis. Cord and membranes healthy.

*Nerves under microscope.*—A number of the tubes are healthy, whilst others have undergone a segmentation of the myeline, and we have *fatty granules, which in some tubes completely fill the substance of Schwann*. He states that when the nerves have been in chromic acid and alcohol all traces of fat and segmentation of the myeline have disappeared, and the axis cylinder is not to be made out. There is proliferation of connective tissue nuclei.

There are no changes in the nerve roots, and the alterations are confined to those parts of the nerve which are distributed to the muscles.

In cases quoted from M. Lancereaux and M. Le Gros, even more of the locomotor ataxic symptoms are described; formication, sharp jerking pains, atrophy of muscles, fleeting ocular troubles, general anæsthesia, and death by asphyxia in one case, and pulmonary apoplexy in the other. Cord healthy. Nerves affected as above.

The chief symptoms are atrophy of the muscles, sensory troubles, which are increased by movement, especially where we have hyperæsthesia in the form resulting from cold, &c. (this is as a rule wanting in the form resulting from lead poisoning, where we may, however, have numbness and cramps), and in the generalised form of the disease the symptoms are those of locomotor ataxia.

Professor Grainger Stewart,<sup>1</sup> describing the changes in the nerves from a case of peripheral paralysis, points out a series of conditions which appear to me to be identical with those I had demonstrated in not only the sensory cranial nerves, but also in the parts of the cord and medulla oblongata, in which I found the myelitic conditions.<sup>2</sup>

These cases then go far to help me in proving that we have in peripheral paralysis a key to the pathology and symptomatology of locomotor ataxia. In Professor Stewart's case of peripheral paralysis we have the breaking up of the axis cylinder to form fatty bodies within the sheath of the nerve. These are seen to be arranged in rows at first, but eventually become discharged into the surrounding tissues, where they are seen as colloid bodies, or are absorbed as fatty particles. The increase of connective tissue is here not so rapid as in the cord, and we observe the

<sup>1</sup> *Edinburgh Med. Journ.* 1881.

<sup>2</sup> I should here point out that M. Gombault, in a "Contribution à l'étude Anatomique de la Névrite parenchymateuse subaiguë et chronique, Névrite segmentaire periaxiale," in *Archives de Neurologie*, 1881, pp. 11-177, describes two conditions of nerves of monkeys affected by lead poisoning:—

(1) Occurring at various parts of the nerve, and affecting several out of a bundle only, commencing at a certain end of the internode, then affecting the other end, and lastly, the intermediate portion. Here there is no change in the axis cylinder, but the myeline undergoes segmentation, and may be broken down into small fatty droplets stained black with osmic acid. In this form the connection with nerve-cell is at no point interrupted, and it may be set up by irritation.

(2) The second form occurs where the above process is extended, and there is interruption at some point between the nerve-cell and the part at which the change is taking place. This interruption may extend from the cell without the first process being brought in at all. In this we have the axis cylinder becoming varicose, and all the other changes, apparently as we have described them, with the exception that M. Gombault holds that the black granules are derived from the myeline.

He insists, however, on the fact that in this form there is gradual loss of function in the nerve fibre, whilst in the first form this is not the case. He speaks of both these processes as "inflammatory" and as "parenchymatous."

preliminary stages more easily and accurately than I have hitherto been able to make them out in the cord. I think, however, that if any one will take the trouble to examine the drawings from sections from this case carefully, and compare them with the drawing given by Professor Stewart, they will find how nearly alike the two processes are.

As yet we have no definite statistics as to the causes of these diseases, but, so far as can be made out, these causes are identical in the two cases. They may both follow acute and chronic disease; they both may be brought on by great exertion of co-ordinating movements, by syphilitic poison, lead poisoning, &c.; whilst, as we have seen, in both, the symptoms are similar. The course may be more or less rapid or extremely chronic in both. One difference should be noted, and that is that in peripheral paralysis, in some cases at least, we have muscular atrophy, whilst in locomotor ataxia proper there is none of this, unless the anterior horns of grey matter become affected. This may be explained by the fact that, in a nerve the sensory fibres are so blended with the motor fibres that, although they are first affected, as evidenced by the acute pain at the commencement of the disease, the motor fibres are secondarily implicated (just as in the case of the fourth nerve in the valve of Vieussens), and we have then atrophy of the muscles to which these fibres are supplied; whilst, when the posterior root zones are affected, the nerve fibres involved are some distance from any motor fibres, and we have consequently no affection or atrophy of the muscles until the disease is somewhat advanced. In the same way we must account for the paralysis in the one case and the want of it in the other. In the early stage of the parenchymatous neuritis we have all the conditions necessary for a locomotor ataxia.

What then are the causes of the ataxic symptoms? The cord is evidently not necessarily a centre for co-ordinating movements, for, as above noted, there is a loss of co-ordinating power in peripheral paralysis, although the cord has been proved to be perfectly healthy.

In the lesion demonstrated it appears probable that we have a sufficient cause for the altered co-ordinating power.

Dr. Brakenridge<sup>1</sup> considers that we have to take into account

<sup>1</sup> *British Med. Journ.* Oct. 11, 1879.

the diminished (not abolished) sensibility to external impressions. This, however, appears to me to be a mere accident, consequent on the condition of the nerves, but not necessary to the want of co-ordinating power. In the same manner it is not necessary, I consider, that the "sense of weight" should be impaired in degree, though this for the same reason is generally the case, and so on with the sensibility of other parts, which Dr. Brakenridge groups under headings of deep parts, periosteum, &c. *It is essential*, however, that the power of accurately localising an impression be lost, *not as to intensity and position, but as to time.*

We have already seen that in an affected portion of nerve fibres, either in the cord or in a nerve trunk, we can observe in the same field ( $\times 800$ ) of very small area that we have axis cylinders in all stages of constriction, from a simple indentation to a complete division. As the constriction becomes more marked the intervening nodules become larger and thicker. We have now our nerves, some healthy and some about to become divided, with all intervening stages (those already divided at the internodes we may leave out of account for the present).

Along the normal fibres a peripheral impulse will be carried at the normal rate of transmission to the cord, medulla oblongata or cerebrum, and a certain effect is produced reflexly if the connecting fibres be still intact, or on the sensorium, if the path be still perfectly clear. Those nerves, however, which have become varicose have an altered conducting power. As the constrictions and enlargements become more and more marked, it is hard to believe that there is no alteration in the conductivity of the nerve.<sup>1</sup> If the conduction be molecular, it would appear that, owing to the frequent concentration and dispersion of energy in its passage along the varicose nerve cylinder, the rapidity with which the impulse passes is considerably de-

<sup>1</sup> Comparing this with the passage of an electric current along a wire, we know that the conductivity of a wire remains the same whether it be "varicose" or not, if the amount of material be the same. That is, the resistance to the current remains the same, with this exception, however, that if any part of the wire be so thin as to be heated to a very high temperature by the passage of the current, there will be an additional resistance set up on account of this raised temperature. In connection with the nerve, we must remember that owing to the imbibition of fluid (Gombault, *loc. cit.*), there is both increased mass of matter in the axis cylinder and alteration in its molecular constitution.



creased, so that an impulse gradually takes a longer and longer time to reach the brain. Along with this we have the altered condition of the quality of the sensation to which allusion has been made. In some fibres, again, the axis cylinder is so broken down that no impulse of any kind can pass. As a result of all this the patient has, as it were, "lost his bearings." His receptive organs may be in perfect order. He is able to translate each impulse as he receives it, and acts on the translation of this in such a manner that if it were normal he would effect his object. This impulse is not normal, it is made up of numerous impressions, all starting at the same time, but all arriving at their destination at very different intervals, so that the patient gets false impressions. He therefore contracts muscles and co-ordinates movements which would serve his purposes well enough if the impressions as he received them had started as such, but which are all far from accomplishing the necessary movements in answer to the impulses as they started from the periphery.

This disorganisation of the rate of passage of sensory impulses is produced all the same whether the nerve be affected in the posterior root zone of the spinal cord in the peripheral termination of an ordinary sensory nerve, or in the peripheral or central terminations of a nerve of special sense. If the root zone be affected in the lumbar region, we have the ataxic gait; if in the cervical enlargement, we have want of power of co-ordinating movements in the hands and arms; if in the nerves coming from these parts, we have just the same symptoms of inco-ordination of movement, &c., such is the case in peripheral paralysis; if, as in the case before us, there is an affection of the auditory nerve, there will be delay in the arrival of an impression arriving at the auditory centre, also an irregularity in the time of arrival, so that there will be a certain amount of hesitancy on the part of the patient (as in this case also) in answering questions. First, from the fact that the impression is delayed as a whole; and, second, because the impulse is delayed irregularly, and it takes the patient some little time to gather the question, and in some cases he can not get the whole of it.

In connection with the changes in the optic nerve, several points of more than ordinary interest crop up. The inco-ordina-

tion in the movements of the eyes is a marked condition where the tabetic changes have passed for any distance beyond the region of the cord or even into the upper part of the cord. This, I think, may be explained as above.

There still remains to be seen whether the change does not manifest its presence in a manner peculiar to this nerve because of the peculiarity of the function of the nerve.

In addition to the constricted field of vision which latterly in this case became marked, there was colour blindness. This has been noted in a considerable number of cases of locomotor ataxia, and appears, according to Erb, to be demonstrated in patients before any limitation of the field of vision.

We have before stated that this varicose condition of the nerves of the retina and the optic nerve has frequently been demonstrated, but we have as yet been unable to find any account of a connection between this condition and colour blindness. Leber<sup>1</sup> has, however, pointed out that the changes in the optic nerve are exactly like those found in the cord (posterior columns); and though I cannot agree with him as to the condition, I do most certainly hold with him as to the similarity of the disease in the two situations. The want of co-ordinating power is here, we think, to be attributed to the unequally delayed impulse, and in like manner it is probable that we have unequally delayed colour-sense impulses, from which we should argue that there would be, as is actually the case, colour blindness.<sup>2</sup>

Dr. Brailey, in his report on colour blindness, states that those people who are much employed in sorting colours, &c., are frequently affected with colour blindness. Dr. Brakenridge<sup>3</sup> states that much exercise, involving great muscular co-ordination (as in the case of a trombone player, who was affected with ataxic symptoms in the arms), will bring on tabes where there is any predisposition to such a condition. So that Dr. Brailey's

<sup>1</sup> Leber, "Ueber graue Degener: der Sehnerven," *Arch. f. Ophthalm.* xiv. 2, 1868, p. 177.

<sup>2</sup> It is a curious fact in connection with the narrowing of the field of vision and colour blindness, that these come on together, as it were, except where violet is concerned (and this is a doubtful exception). As the field of vision narrows, it narrows for colours also, green going first, red and orange next, and lastly yellow and blue. Violet is seen by the most central part of the retina.—*Charcot*.

<sup>3</sup> *Loc. cit.*

facts support the theory that colour blindness as a symptom of locomotor ataxia, or even in other diseases, is due to a neuritic condition of the optic nerve, the principal feature of which is a varicose or moniliform condition of the axis cylinders of the optic nerve fibres or of the retina. In the case under consideration symptoms and altered conditions as described were undoubtedly present.

The above facts, taken for what they are worth, I think, at any rate go to support the theory (so far advanced from the clinical point of view), that peripheral paralysis, as affecting the sensory nerve terminations in the limbs, and that involving the terminal portions of the sensory cranial nerves or the trunks of these sets of nerves themselves, is accompanied by changes similar to those found in locomotor ataxia.<sup>1</sup> The inflammatory process, whether in the cord or in the peripheral nerves, appears to commence at a nutrient centre, from which centre the nerve originally grows. In the case of the peripheral sensory nerve, which is developed centripitally, the termination is in the skin or muscle, from which point a parenchymatous neuritis is set up, and ataxic symptoms are developed. In the cord, as held by Flechsig, all the tracts which are affected in tabes dorsalis are also developed centripitally; the posterior root zones, the fasciculus gracilis, and the direct cerebellar tracts developing from below upwards and undergoing neuritis and degenerative changes in just the same manner from the posterior root zones upwards in this tabetic or ataxic disease. It is, in fact, just the reverse of the process noted in progressive muscular atrophy, where the columns affected develop and degenerate centrifugally.

I must now confine myself to giving a sketch of the localisation of the disease as affecting the medulla oblongata especially, leaving the further details to be discussed when the whole case is published by Professor Grainger Stewart.

In this case, as we have seen, the fifth nerve is affected in its peripheral extension; but had not this been so, we can at once

<sup>1</sup> The notes from which the above report was written were made during the summer of 1880, when the only cases I had for comparison were those mentioned by Joffroy. I have, however, since then availed myself of the case given by Professor Grainger Stewart to confirm my observations, as I think they most undoubtedly do, more especially as he states that he has arrived at the same point from a consideration of the clinical aspects of the disease solely.

see that the ascending root, placed as it is as the "cap" of the posterior horn of grey matter, between the posterior root zone and direct cerebellar tracts, could scarcely escape implication, and to this involvement of the fifth nerve, as Pierret points out, many of the reflex paralysis of the face may be attributed.

Another mass of grey matter in a position to be affected is that portion of the central grey matter cut off by the decussating fibres from the posterior roots which go to join the anterior pyramids on their way to the optic thalamus. This grey matter is continued from the posterior horns of central grey matter into the sensory nuclei of the mixed nerves (spinal accessory, pneumogastric and glossopharyngeal), so that we cannot but come to the conclusion (in spite of Erb's statement to the contrary) that these nerves, connected as they are with the posterior grey matter and these decussating fibres, become involved as the disease passes upwards. It is to this fact, in all probability, that we may refer the symptoms, grouped under the name of "gastric crisis" by the French writers, and the accompanying changes in speech which are so characteristic of this disease, and of general paralysis of the insane, in which one so often finds tabetic changes in the posterior columns in the dorsal region.

In the case before us there are undoubtedly affections in these areas, not as yet very pronounced, but amply sufficient to account for the lancinating pains referred to the occipital region, and for the great disturbance which takes place in the area presided over by the vagus, &c.

Along with the affection, either direct or reflex, of the hypoglossal nerve (the decussating posterior root fibres passing directly external to its nucleus), there will, in all probability, be affection of the nerve of Wrisberg or chorda tympani.<sup>1</sup> (See case when published.) In connection with this region, too, and pointing to the close relation of the nucleus of the sixth nerve with the glossopharyngeal nerve, we must mention the band of fibres described by Clarke as passing between these nuclei.

<sup>1</sup> In a section through the medulla oblongata at the level of the exit of the nerve of Wrisberg, we see an increase of connective tissue. Nerve-cells of the anterior nucleus of the auditory nerve are deeply pigmented, and a considerable number of colloid bodies are seen running in the direction of the fibres of the nerve of Wrisberg.

The nuclei of the vagus and spinal accessory nerves, prolonged as we have seen from the posterior grey matter of the cord, are in close connection with the band of Stilling. This band is prolonged downwards near the root of the fifth nerve, with which it appears to be intimately related.

So far we have traced out the myelitic change in the posterior pyramids, in part of their extent, we have also seen it passing upwards to the restiform bodies, after passing round the inferior or ascending root of the fifth nerve; further, briefly, it runs upwards into the valve of Vieussens, a few altered fibres also running in the walls of the fourth ventricle at this point, and a few of the altered axis cylinders may be traced in the arms of the posterior tubercles or "deep bands" of Reil (in a section made through the posterior corpora quadrigemina) as they pass outwards, and also as they decussate in the roof of the aqueduct of Sylvius, after which we may follow the changes in these bands as they course through the tubercles to which they pass. There are evidences of the disease in the raphe.<sup>1</sup> In the sixth nerve there is no trace of disease, so far as I can make out, in any part of its course, though the reflex chain, of which it forms a link, is broken by the changes which have taken place in the fifth nerve. The *striae acusticae*, as we should expect, are markedly involved.

It will now be necessary to call upon this anatomical distribution of the disease in the medulla oblongata and mid-brain to aid us in explaining certain phenomena met with in the ataxic condition.

In the limbs it has been pointed out that there is a disturbance of the ordinary relations between sensation and motion, and the reflex actions "are put out of joint." Now, in the case of the movements of the eyeball, we have two nerves of sensation—the optic, a nerve of special sense, and the fifth, a nerve of common sensation. In this case both are affected, in consequence of which affection result the symptoms squinting, irre-

<sup>1</sup> Similar changes are readily made out in the floor of the aqueduct of Sylvius. Here, too, a number of the cells are more darkly pigmented than usual, and, at the same time, more granular-looking. The pigment is collected around the nucleus of the cell, or may be prolonged into one of the processes. Some of the cells in this position are undoubtedly undergoing some fatty degeneration.

gular movements of the eye, and imperfection of the conjunctival reflex; all of which are due, I think, to the irregular conduction of special or common sense impulses along one or other of the nerves. The extra ocular movements are governed by the third, fourth, and sixth nerves, whose nuclei have been put out of connection with the optic and fifth nerves, or, at all events, the sensory portion of the reflex chain of this group has been seriously disorganised.

In the same way are affected certain intraocular movements controlled by the third nerve on the one hand, and the optic nerve as the sensory portion, on the other. Dr. Buzzard<sup>1</sup> suggests that in all probability the Argyll Robertson phenomena were due to the abolition of the reflex action of the pupil, the voluntary motor action being retained.

Our facts coincide most accurately with this theory. The area in which the fibres connecting the corpora quadrigemina with the third nerve and its nucleus is, as we have seen above, markedly infiltrated with the colloid bodies, the result of breaking down of the axis cylinders, and in this region, too, are found the swollen axis cylinders; but at no point have we been able to find any change in the superior motor area, in which are the fibres running from the cerebral hemispheres to join the third nerve.

Under the influence of light, this myosis in most ataxic cases remains perfectly passive. When the light falls upon a perfectly healthy retina, and is conducted by a healthy optic nerve, there is an immediate but purely reflex contraction of the pupil; that is, without any effort of will on the part of the person.

The light falling on the retina forwards an impulse along the optic nerve, through the optic thalamus, corpora geniculata, and corpora quadrigemina (anterior, and perhaps also posterior, of these) to the nucleus of the third nerve, from which a return impulse is set up, and we have contraction of the pupil. In the ataxic condition there is a flaw in the sensory path, or what corresponds to the posterior nerve-roots; there is a break in the reflex circle, and there is no contraction of the pupil, just as there is no tendon reflex in the knee.

If a healthy person is directed to look at a distant and then at

<sup>1</sup> T. Buzzard, *Lancet*, 1880.

a near object, rapid dilatation or contraction of the pupils is observed as the eye accommodates itself to the distances. In thus bringing about accommodation of the eye to the distances, the person under observation will be aware that he is making some effort. He is told to look at an object, and he directs his attention to that object. He is not now a mere receptive being, but he is making voluntary efforts to attain a certain end. There is, in fact, an impulse from the higher or voluntary motor centres brought into requisition, and the act is no longer reflex. In an ataxic patient the same holds good. Tell him to look at a near object, and the pupil immediately becomes contracted, though this contraction is most frequently slow and often very irregular, from which fact we deduce that the contraction is not purely voluntary. It is voluntary in so far as it is controlled by the higher motor centres, but it is reflex as far as the optic nerve plays any part in the process in conveying an impression of the object, its size, distance, &c., to the sensorium.

Take as an illustration of what I mean, the case of an ataxic patient attempting to touch a certain point. Certain impulses start from the retina to his sensorium. They start at the same moment, but those passing along certain fibres of the optic nerve are delayed longer than those passing by others. Others, again, are altogether interrupted, so that eventually a very mixed or disordered impression arrives at the destination. This disordered impulse the patient is unable to interpret correctly, and as a consequence of this the wrong muscles are thrown into action, or the right muscles act in excess or fall short of what is required to accomplish the end in view.

This, of course, applies to the ciliary muscle amongst others. It may be contracted voluntarily, but because an impulse is sent to the sensory centre, and as that impulse passes slowly and irregularly, there is a delayed and irregular action of the pupils, as by volitional efforts there is an attempt made to accommodate the eye to an impulse which, as it arrives, is deceptive.

In this case, then, we may sum up as follows:—

1. There is a diseased condition of the sensory tracts of the cord, posterior nerve roots, posterior columns, and the direct cerebellar tracts. So far it presents the conditions requisite for

the appearance of locomotor ataxic symptoms, and it seems probable that there was a distinct origin of the disease either in or near the posterior root columns, followed by an ascending degeneration.

2. The sensory cranial nerves are similarly affected (with the doubtful exception of the olfactory nerve), which brings the disease into the region of peripheral paralysis, the diseased condition here commencing at the peripheral termination of the nerve.

3. The two processes have met in the fifth nerve, and probably also in the continuation of the optic tracts.

4. We say that there are two sources of origin, but it really comes to this, that the posterior or sensory tract of the whole cerebro-spinal nervous system is affected, though in the case of the cranial sensory area the peripheral nerves are first implicated.

In connection with the above I may point out that the arms of the posterior tubercles of the corpora quadrigemina were at one time supposed to run to the posterior columns of the cord. A later theory is, that they are motor bands which pass to the antero-lateral columns of the cord with the motor field. We found, however, that these bands were affected at their decussation and also lower down, from which I venture to suggest that the old theory is probably the correct one, that these bands are sensory, and are connecting links between the optic nerves and tracts with some region in the cord, through which movements purely reflex may be brought about more rapidly. In any case they form, as it were, a common ground for a meeting of the diseased process extending from the cord on the one hand, and the optic nerve on the other. The anterior bands appear to be undoubtedly motor, and run to the motor columns of the cord.

5. The process is a myelitis, which commences primarily in the nerves, probably, in this form, in the axis cylinders.

6. It is connected with the degeneration of the nutrient and functional centre of the nerve.

7. With it may be associated a secondary condition due to irritation, which may take the form of the peri-axile neuritis.

8. The symptoms found in ataxia are due to the altered condition of the axis cylinder of the sensory nerves, the altered condition causing a break, either partial or complete, in the reflex circle.



THE ACTION OF SALINE CATHARTICS. By MATTHEW HAY, M.D., *Demonstrator of Practical Materia Medica in the University of Edinburgh.*

(Continued from page 282.)

SERIES OF EXPERIMENTS, B.

ON the effect of the salt on the intestine, when injected directly into the viscus after the method of Colin and Moreau; with analyses of the fluid, as to the salt it contained, and as to its digestive and other properties.

The method practised by Colin and Moreau, with its modification by Brieger,<sup>1</sup> I have already described. Twenty per cent. or even stronger solutions of the purgative salt, sulphate of soda, or sulphate of magnesia, were used by all the observers who employed this method. In every case there was a considerable flow of fluid into the injected coil, whether the experiment was made on the horse (Colin), the dog (Moreau, Vulpian, and Brieger), the cat (Brunton), or the rabbit and frog (Böttger).

I have repeated these experiments with certain important modifications. It is as yet the only method by which direct proof has been furnished of the salt exciting secretion within the intestines. And it were desirable to know, especially after a consideration of the experiments of Series A. of this investigation, in conjunction with the observations of Thiry, Schiff, and Radziejewski,<sup>2</sup> whether the method employed by Colin and Moreau was reliable. To what extent was the secretion due to the nature of the operation required,—the exposure of the intestine, and its irritation by ligatures, &c.? Perhaps it was caused by the irritant action of the unusually strong solution of the salt injected. Solutions of less strength than 20 per cent., excepting in a single experiment of Brieger's,<sup>3</sup> were not at any time employed; and it is well known that a strong solution of a salt will irritate living tissues, whilst a weaker solution will exercise no such effect. For example, a strong solution

<sup>1</sup> P. 247.

<sup>2</sup> Pp. 250, *et seq.*

<sup>3</sup> *Infra*, p. 396.

of common salt will irritate and inflame even the skin, whilst a 1 or  $\frac{1}{2}$  per cent. solution is so bland as to be the fluid commonly used in physiological experiments for the immersion of delicate tissues and organs whose vitality we wish to preserve uninjured as long as possible. It is certain that the purgative salt never, in ordinary circumstances, reaches the intestines so concentrated as would be implied by a 20 per cent. solution. It is rarely swallowed in a state of such concentration, and in the stomach and intestines it at once mingles with a large quantity of alimentary fluids which quickly dilute it. To settle these and other points is the aim of the present series of experiments. And although I have already thrown aside the theory of purgative action with which I started this investigation, and which I have discussed at length in the preceding experiments, I was still endeavouring, at the time the following experiments were undertaken, to ascertain what of truth or error there might be in this theory which was originally suggested by Buchheim, and brought prominently under my notice by Professor Schmiedeberg. The remembrance of this will sometimes account for the arrangement of my experiments.

In this series of experiments, excepting a few rabbits, cats were always employed. They were more readily procurable than dogs, and in the structure of their alimentary system they more closely resemble man than do rabbits. Moreover, they enabled me to compare my results with those of at least one (Lauder Brunton) of the preceding observers. My method of procedure was as follows:—The animal having been anæsthetised with a mixture of chloroform and ether (1 to 3), which I found to act four or five times as quickly as pure ether, and much more safely than pure chloroform, was attached to a holder placed over a large oblong porcelain tray containing water heated by a bunsen flame. The steam arising from the water in the tray enveloped the trunk of the animal during the operation, and helped to maintain the temperature and moistness of the exposed intestine. The operation consisted in exposing the intestines by a longitudinal incision in the linea alba nearly midway between the processus xiphoideus and the symphysis pubis, and from one to two inches in length. Too short an incision is not advisable, as it is apt to necessitate

undue pressure in returning the intestine into the abdomen. Generally not more than a drop of blood was lost in making the incision. The omentum having been carefully drawn up by introducing the finger or the handle of the scalpel, so much of the intestine was withdrawn from the abdominal cavity as it was desired to operate upon. The part selected of the small intestine was generally situated near to the cœcum. As in each experiment no food had been given since the previous afternoon, the intestines were usually perfectly empty, or contained at most a little gas and a few brownish shreds of incompletely digested food. The diet of the cats, as in the preceding series of experiments, consisted of boiled flesh, along with what of the water in which the meat had been boiled they chose to take. Each animal was fed regularly twice a day for a week before the experiment, and the healthy condition of its alimentary system was ascertained from the character of its stools. To return to the steps of the operation, a measured portion of the exposed intestine was ligatured off by means of fine caoutchouc tubing, such as is attached to the cannula for draining anasarcaous tissues. This form of ligature was sufficiently tight to prevent the passage of fluid through the constricted part of the intestine, and at the same time was sufficiently yielding as not to cut the walls of the intestine, which ordinary thread ligatures are rather apt to do. The ligature was always passed through the mesentery as close as possible to the intestine in order to avoid including the large anastomotic branches of arteries, veins, and lacteals which fringe the mesentery. At the extreme ends of the ligatured coil two short longitudinal incisions were made into its lumen. These were generally followed by a little bleeding. Through the upper of the incisions was introduced a glass cannula connected by an india-rubber tube with a large ball syringe filled with a  $\frac{1}{4}$  per cent. solution of chloride of sodium at a temperature of from 35° to 40° C., by means of which the coil was gently but thoroughly washed out as indicated by the purity of the fluid escaping from the lower incision. Due care was of course taken to prevent any of the washings passing into the peritoneal cavity. The small intestine of the cats was found almost invariably to contain several tape worms, and the removal of these with the salt solution

sometimes prolonged a little this part of the operation. The object of thus carefully cleansing the intestinal loop was to ensure that the secretion excited by the afterwards injected purgative would be free from admixture with the biliary and pancreatic secretions and with the products of digestion. In a few experiments where it was desirable to avoid much manipulation of the intestine, and where it was not necessary to ascertain the nature of the secretion, the washing was not practised. A ligature was now placed round the coil, under and close by the upper opening, and the loop was emptied of its salt solution by gentle stripping with the fingers. Another ligature was then applied immediately above the lower incision. The washed empty portion of intestine included between the two last ligatures was next divided into three equal portions by means of other two ligatures, and into the central of the three was injected the solution of the purgative salt. For this purpose a syringe provided with a fine steel cannula was employed, its point being pushed through the wall of the intestine. The viscus was then returned within the abdomen, and the wound in the linea alba was carefully closed with a number of closely set ligatures to prevent the hernial protrusion of any portion of the gut, which was more apt to happen from the not unfrequent occurrence of a little vomiting after the operation—the effect probably of the anæsthetic as well as of the nature of the operation. The animal after being released from the holder was enveloped in a warmed cloth and placed in a basket near the fire. Half-an-hour was generally required for it to regain sufficient consciousness and strength as to be able to sit up; and in half-an-hour more it looked so well—apparently free from pain and even bright and lively—that it would have been difficult to say that the animal had been operated upon. An occasional vomit, and that only in some experiments, was the sole evident indication of its condition. None of the animals were kept so long as to permit of peritonitis with its accompanying pain occurring. Finally, the animal was killed by strangulation, aided by a sharp blow on the head with a mallet.

The method of operation occasionally varied from what I have described, but where any modification was practised it will be recorded in the protocol of the experiment. The time occupied

by the whole operation—from the removal of the intestine from the peritoneal cavity to its replacement—rarely exceeded five minutes. The operation was conducted with the greatest possible expedition in order to avoid the irritation of the intestine resulting from its prolonged exposure.

In arranging my experiments I had some difficulty in deciding what length of intestinal loop I ought to employ. A long loop, otherwise preferable, was objectionable on account of its requiring the exposure and manipulation of a large portion of the abdominal contents. I therefore chose for my first experiments a short loop. Lauder Brunton, probably for a similar reason, employed also a short loop. Most of the other observers, however, made use of loops of considerable length.

*Experiment XXXIII.*—Cat, female, weighing 1.59 kilogrammes. Operation performed exactly as described above—coil of small intestine washed out with warm  $\frac{3}{4}$  per cent. solution of chloride of sodium, emptied and divided by ligatures into three equal portions. Into the central of these was injected 1 c.c. of a 10 per cent. solution of chemically pure crystalline sulphate of soda. The temperature of the solution was about that of the animal ( $38^{\circ}$  C.). The cat was killed *five hours* afterwards, and the abdomen at once opened, and the condition of the intestines noted.

**AUTOPSY.**—The condition of the loops was as follows:—

Upper control contained 0.1 c.c. of very viscid, colourless, opaque fluid. Lower control contained 0.3 c.c. of a similarly viscid and opaque fluid (slightly reddened).

Injected loop contained 3.45 c.c. of a transparent, colourless fluid, in great part limpid, although mixed with a small quantity of viscid mucus, and with some whitish flocculi. Reaction, alkaline. The reaction of the injected saline solution was, of course, neutral. Caustic potash, with the aid of a little heat, dissolved the viscid flocculent deposit almost completely, which was reprecipitated by acetic acid, and was not soluble to any visible extent in excess of the acid. It, therefore, consisted largely of mucus. The acetic acid filtrate gave no precipitate, but merely a distinct opalescence, with a drop of a solution of ferrocyanide of potassium. A trace of albumen was therefore present. Other reagents for albumen led to the same conclusion.

Microscopically, the flocculent deposit was seen to be composed of mucous corpuscles and mucin, with a number of epithelial cells, and a quantity of granular matter, part of which, from its high refractive power, appeared to be oily in nature. The mucous corpuscles were distinctly nucleated, and had a granular protoplasm.

The mucous membrane of all three loops was perfectly pale, unless in the neighbourhood of the ligatures, where there were very slightly congested zones.

The injected loop measured 6 cm. long, and each of the controls 5 cm. The operated part of the intestine was 15 cm. from the cœcum and 87 cm. from the pylorus. The total length of the small intestine was 122 cm.

A 10 per cent. solution of sulphate of soda appeared, therefore, to be as capable of causing a flow into the intestinal loop in which it had been placed as the 20 per cent. solution used by all previous investigators. An experiment of exactly the same nature was made upon a rabbit.

*Experiment XXXIV.*—Rabbit, female, weighing 1·47 kilogrammes. Loops prepared as in previous experiment, and 1 c.c. of a 10 per cent. solution of sulphate of soda injected into the middle loop. Killed after five hours.

*AUTOPSY.*—Upper control contained 0·15 c.c. of bloody mucus. Lower control was empty.

Injected loop contained 3·55 c.c. of a colourless, transparent fluid, partly limpid, partly viscid and glairy; no odour; alkaline reaction; specific gravity, 1·0130. It contained a trace of albumen, but no glucose or other material capable of reducing oxide of copper.

Its digestive powers were ascertained. To a portion of it was added a very small piece of fresh fibrin, and the mixture placed in a digesting oven at a temperature of 40° C. After seven hours the fibrin was not dissolved and scarcely diminished in bulk; the fluid did not give the peptone reaction with sulphate of copper and caustic potash. Another portion of the original fluid was mixed with a boiled solution of starch, which was previously ascertained to contain no glucose, as common starch often does, and set aside for a night in a warm place. Next day the mixture gave abundant evidence of the presence of glucose, or rather maltose, according to the researches of Musculus and Von Mering.

The fluid taken from the loop presented the same microscopical characters as that of the previous experiment.

The mucous membrane of the loops was not congested.

The injected loop measured 7½ cm. The seat of the operation was in the upper part of the ileum. The whole length of the small intestine was 258 cm.

The action, then, of a 10 per cent. solution of the salt is much the same in the rabbit as in the cat, and this was confirmed by other experiments.

According to Brieger,<sup>1</sup> who injected a ½ per cent. solution of sulphate of magnesia, as well as a 20 per cent. solution, the weaker solution is absorbed. I have just ascertained that a 10 per cent.

<sup>1</sup> Brieger, *Archiv. f. experim. Patholog. u. Pharmacolog.*, Bd. viii. S. 358.

behaves like a 20 per cent. solution, inasmuch as it excites a flow of fluid into the intestine. There must, therefore, exist between the  $\frac{1}{2}$  per cent. and the 10 per cent. a strength of solution which will neither excite secretion nor be absorbed. What this strength is the next experiments help to elucidate.

*Experiment XXXV.*—Cat, male, weighing 1·84 kilogrammes. A set of loops was prepared on both the ileum and the colon. Into the central of each was injected 1 c.c. of a  $2\frac{1}{2}$  per cent. solution of sulphate of soda. Killed *three hours* afterwards.

*AUTOPSY.*—All the coils were perfectly empty, and the mucous membrane of each was quite pale. The injected ileac loop was 6 cm. long; that of the colon, 2 cm. The ileac loop was 20 cm. from the cæcum and 71 cm. from the pylorus. The injected loop on the colon was 4 cm. from the cæcum.

A  $2\frac{1}{2}$  per cent. solution is absorbed whether it be injected into the small intestine or the colon. Will a 5 per cent. solution be likewise absorbed?

*Experiment XXXVI.*—Young male cat, weighing 1·41 kilogrammes. An injection of 1 c.c. of a 5 per cent. solution of sulphate of soda was made as in the previous experiment into the large as well as into the small intestine. There were the usual three loops on the former. On the latter there was only one, but with a couple of ligatures at each extremity to shut off the openings by which the loop had been washed out with the chloride of sodium solution. Killed at the end of *five hours*.

*AUTOPSY.*—The controls were empty. The injected loop of the small intestine contained 0·3 c.c. of a transparent, colourless, somewhat viscid fluid, with a small quantity of an opaque, yellowish deposit. Reaction, alkaline. In the loop of the colon were found 3·5 c.c. of an alkaline transparent, colourless, extremely viscid fluid, strikingly resembling in consistence and appearance white of egg; it was considerably more viscid than the fluid from the loop of the small intestine; specific gravity, 1·0154. A portion of it was opaque, yellowish-white and flocculent, and looked as if it had formed a layer over the mucous membrane of the loop. This fluid contained no appreciable trace of albumen, although tested with all the common reagents, as nitric acid and ammonia, sulphate of copper and caustic potash, acetic acid and ferrocyanide of potassium. It did not, although appearing to contain a considerable quantity of mucin, give much turbidity with excess of acetic acid. It digested starch with ease.

Microscopically, the fluid from the colon contained throughout a large number of motionless bacteria. The opaque sediment consisted largely of mucous corpuscles, also of epithelial cells of various shapes,

some goblet-shaped, others breaking up or degenerating. There was a large quantity of granular debris, and a few crystals of phosphate of lime and of leucin and tyrosin, which had probably not been removed from the mucous membrane by the washing with the solution of common salt.

The mucous membrane in the loops, both of the small and of the large intestine, was pale and uncongested.

The operated part of the ileum was 5 cm., and that of the colon 3 cm., from the cæcum. Both of the injected loops were 3 cm. long.

The ileac loop was, therefore, half the length of the injected loop in Experiment XXXIII., making the quantity of the salt, in relation to the length of intestine acted on, the same in both cases. Yet at the end of the same time the 5 per cent. solution had diminished to one-third of its original bulk, while the 10 per cent. solution had increased to three and a half times its original quantity.

As the salt had produced a remarkable flow of fluid from the colon as compared with the small quantity obtained from the ileum, it was desirable to know if this difference were constant; for it pointed to the salt in ordinary purgation procuring its fluid in greatest part from the colon.

*Experiment XXXVII.*—Cat, female, weighing 2·835 kilograms.

Three loops were as usual formed on the ileum, but instead of the central the two lateral were injected, the one with 1 c.c. of a 5 per cent. solution, the other with 2 c.c. of a 2½ per cent. solution of sulphate of soda, the central loop being preserved in this instance as a control. Both injected loops, therefore, received the same amount of the salt. On the colon were formed two adjacent loops, which were injected with the same quantities of the same solutions as the ileac loops. The cat was killed *two hours* afterwards.

#### AUTOPSY.—

Ileum—	5 per cent. solution had increased to	1·4 c.c.
Colon—	" "	1·2 "
Ileum—	2½ per cent. solution had "sunk to	1·0 "
Colon—	" "	1·2 "

The central control on the ileum was quite empty.

The injected loops of the ileum measured 5·7 cm. (5 per cent.) and 5·6 cm. (2½ per cent.). The two loops of the colon measured each 3 cm. The circumference of the small intestine, obtained by measuring its diameter while distended with fluid under a moderate pressure was 4·2 cm.; of the large intestine, 8·8 cm. The operated part of the ileum was 49 cm. from the cæcum. The total length of the small intestine was 151 cm.



All the fluids were faintly but distinctly alkaline, and all were colourless, transparent, and viscid, those of the colon being the more viscid. All digested starch, and contained very little albumen.

This experiment, in opposition to the previous experiment, seems to indicate that the ileum and colon behave much alike towards equally strong solutions of the sulphate. As the determination of this point is of interest, I shall endeavour to show wherein may lie the reason for the conflicting results of these two experiments. The two essential differences between the conditions of these experiments are to be found in the length of the ileac loop and in the time during which the salt was permitted to act on the intestine. As in both experiments the same quantity of a 5 per cent. solution of the salt was injected, it is obvious that the proportion of ileac mucous membrane to the salt solution must have been greater in the one experiment than in the other. And an alteration in this relation may materially affect the amount of secretion excited by the salt. This remark applies equally to a comparison of this relation in the colon with that in the ileum. Assuming the circumference of the corresponding viscera to be the same in both experiments, the extent of mucous membrane in the injected ileac loop of Experiment XXXVI. was 3 cm. (length)  $\times$  4.2 cm. (circumference) = 12 sq. cm.; in the colon it amounted to 3 cm. (length)  $\times$  8.8 cm. (circumference) = 26.4 sq. cm. In the next experiment (XXXVII.) the mucous membrane of the ileac loop injected with the 5 per cent. solution measured 5.7 cm. (length)  $\times$  4.2 c.m. = 23.8 sq. cm., and of the corresponding loop of the colon, 3 cm.  $\times$  8.8 cm. = 26.4 sq. cm. Thus in the latter experiment the two mucous membranes were of nearly equal superficies, in the former that of the ileum was only half that of the colon, and in both experiments the mucous membranes of the colon were of equal extent. In three of the loops, therefore, was the membrane of nearly the same size, while in the fourth it was much smaller. Where those of the ileum and colon are equal (Experiment XXXVII.) the effect of the salt solution is much the same on both. Where they are unequal (Experiment XXXVI.) the smaller the extent of membrane exposed to the action of the salt, the less appears to be the amount of the secretion excited. Such a difference of effect is

what might well be expected. But the next experiment, undertaken with the view of settling this point, seems to indicate otherwise.

The other element of difference in the conditions of the experiments which I am now comparing was that of time. This seems clearly to account for the larger quantity of secretion found at the end of five hours in the colic loop (Experiment XXXVII.) as contrasted with that recovered from the corresponding loop and the ileac loop after the lapse of two hours (Experiment XXXVI.). To what extent it may have influenced the wide difference between the 5 per cent. solutions in the ileac loops, apart from the inequality of their length, will shortly be discussed.

One other point brought out in the last experiment is that the salt dissolved so as to form a  $2\frac{1}{2}$  per cent. solution acts on the same length of intestine differently from the same quantity of salt as a 5 per cent. solution. The former solution is gradually absorbed, while the latter increases in bulk, although, from the inequality of their quantities when injected, they do not materially differ in amount at the end of two hours.

*Experiment XXXVIII.*—Cat, male, weighing 3.63 kilogrammes.

Three contiguous loops were formed on the ileum by ligaturing as in previous experiment, two of equal length, but the third or upmost twice as long as each of the others. Into the long loop was injected 1 c.c. of a 10 per cent. solution of sulphate of soda, and into the lower of the short loops a similar quantity of the same solution. The animal was killed *two hours* afterwards.

**AUTOPSY.**—Long loop contained 1.44 c.c. of a very viscid fluid; short loop, 3.2 c.c. of a fluid, although viscid, not so much so as that in the long loop. Both fluids were colourless and partly transparent, partly flocculent and opaque. The reaction of both was alkaline.

The long loop measured 11.6 cm. in length, and the short loop, 6.1 cm., and was situated about 6 cm. from the cæcum.

The result of this experiment is not in harmony with the conclusion tentatively expressed, that the greater the extent of mucous membrane exposed to the action of the salt, the less is the secretion, and which was directly inferred from the results of the two preceding experiments. With these opposing results I deemed it necessary to obtain confirmation of the one or the other by another experiment made under conditions slightly

modified from those of the preceding experiment: the time was extended to five hours, and the colon was likewise ligatured and injected.

*Experiment XXXIX.*—Cat, weighing 2.36 kilograms.

Five loops were formed on the ileum, two for injection and three for controls, and they were so arranged alternately that an injected coil lay between two controls. Into the longer of the injected loops was placed 1 c.c. of a 10 per cent. solution of sodic sulphate. Into the shorter loop was also injected the same quantity of the salt solution. On the colon was formed a single loop, accompanied by the usual four ligatures, and into it was injected 1 c.c. of the same solution. The cat recovered well from the effects of the anæsthetic, and did not vomit. The intestines contained no tape-worm, so that the washing-out was quickly accomplished, and the operation completed in a few minutes. The cat was killed at the end of five hours.

*AUTOPSY.*—Condition of ileum:—Long loop contained 0.1 c.c. of an extremely viscid fluid; short loop, 0.88 c.c. of a less viscid, slightly yellowish, gruel-like fluid. Both were alkaline in reaction.

In the loop of the large intestine were 1.88 c.c. of an excessively tough, glairy fluid, almost tougher than white of egg, and mixed with greyish slimy flakes; reaction alkaline.

The longer of the ileac loops measured 14 cm., the shorter 8 cm. The loop on the colon was 5.2 cm. long.

In so far as the length of intestine affects the amount of the secretion, the result is the same as that of the preceding experiment. The larger the loop exposed to the action of a given quantity of the salt solution, the less is the quantity of the secretion, and we may now, therefore, regard this as an established fact.

Here also as in Experiment XXXVII., the fluid in the colon exceeded the quantity of that in the ileum, without that the extent of its mucous surface was less; the fluid was likewise of extreme viscosity. This affords confirmation of the statement met with in some physiological works,<sup>1</sup> that the secretion of the colon contains more mucus than the secretion of the small intestine. The larger amount of the colic than of the ileac fluid is due either to the secreting glands of the large intestine being more susceptible to the action of the salt than those of the small intestine, or to some other changes of which I have not as yet taken cognisance. From a comparison of Experiment XXXVII. with Experiments XXXVI. and XXXVIII., we may conclude

<sup>1</sup> Heidenhain, *Hermann's Hdbuch. der Physiologie*, Bd. v. Th. 1, S. 168.  
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that if the extent of mucous membrane acted on by the salt be nearly equal in the colon and in the ileum, then, if the action of the salt be interrupted at the end of two hours or so, the quantity of secretion will be found to be much alike in both; but if the salt be allowed to act for five hours, then more fluid will have been secreted in the colon than in the ileum. The explanation is tolerably simple. In both secretion occurs with equal rapidity during the first two hours or until the salt solution within the intestine is so diluted as no longer to possess excito-secretory power. This point having been reached, absorption begins; but the fluid of the colon containing much more mucin than that of the small intestine is absorbed more slowly, and consequently, at the end of five hours, remains, relatively to the ileac secretion, large in quantity. It is highly probable that mucin is hardly, if at all, absorbed; the water in which it is dissolved alone being removed, a fact to which the viscosity of those fluids which had greatly diminished in quantity point (*e.g.* Experiment XXXIX.), and which is in accordance with what is known of the physiology of digestion.

But we are not done with the apparent contradictions which the results of these experiments present. Much must be allowed for differences in the constitution of the various animals, in the healthy condition of the viscera, and in the effect of more or less manipulation of the exposed gut. If, however, all these conditions be exactly alike in each experiment, then, regarding the body as a mere machine for the time being, the result of each experiment should be in strict accordance with the known and intended alteration of these conditions. And if we have a discordant result where the conditions are generally good, we must search for the explanation in some unheeded difference in the manner or mode of the experiment. These remarks apply forcibly to Experiments XXXIII. and XXXIX., whose results are strikingly at variance. In the former, after the injection of 1 c.c. of a 10 per cent. solution of a sulphate of soda into a certain length of the ileum, by the end of five hours the fluid has increased to 3.5 c.c. In the latter the same quantity of the salt solution in the same time, placed in a loop of nearly equal length in a similar portion of the intestine, has diminished to 0.8 c.c. The result of Experiment XXXIII. was fully con-

firmed by other experiments whose protocols I have not given, and where the ileum alone was ligatured and injected. The sole difference between the conditions of the two experiments lies in the greater number of ligatures applied to the intestines. The more numerous injections made in Experiment XXXIX. can be of little consequence. That this difference is sufficient to account for the opposing results is amply demonstrated by the succeeding experiments, which, at the same time, prove that the result of Experiment XXXIX. was not exceptional. They were designed for the purpose of ascertaining the action of the salt in different portions of the small intestine before I had perceived the explanation of the difference alluded to. A loop on the duodenum, jejunum, and ileum was injected, and associated with each injected loop were five ligatures—the usual two at each end, binding off the openings through which the loop was washed, and a fifth for the purpose of excluding from the loop the puncture made by the cannula of the syringe; for I had sometimes observed a very slight oozing from the puncture when the loop was largely distended with secretion. There was, therefore, a large number of ligatures. The usual washing out of the intestine was not practised.

*Experiment XL.*—Cat, female, weighing 2.95 kilograms.

A loop was formed in the manner described in each of the three divisions of the small intestine—the duodenum, the jejunum, and the ileum. Into each loop was injected 1 c.c. of a 10 per cent. solution of sulphate of soda. Killed at the end of five hours.

*AUTOPSY.*—The quantity of fluid in the various injected loops was as follows:—

Duodenal loop contained	1.1	c.c.
Jejunal                    "	1.05	"
Ileac                     "	1.1	"

The duodenal fluid was rather limpid, of a yellowish-white colour, and contained a finely divided whitish deposit which was not, as usual, tough and slimy. The jejunal fluid was tinged with a small quantity of blood, and was rather more viscid than the duodenal fluid, and contained some whitish flocculi. The ileac fluid was transparent, colourless, and more viscid than any of the others, with a whitish tough deposit. The reaction was in every case alkaline; and all contained little albumen, and digested starch readily, the duodenal being the most active.

Each loop measured from 6 to 6.5 cm. in length. The duodenal loop was 15 cm. from the pylorus. It was difficult to place it nearer

the stomach without injuring the pancreas, one lobe of which in the cat is closely adherent to the upper part of the duodenum for the greater part of its length. The jejunal loop was midway between those of the duodenum and ileum, the latter being 8 cm. from the cœcum.

In so far as the immediate object of this experiment was concerned, it led to the conclusion that the salt acted equally on all parts of the small intestine.

It at the same time confirmed the result of Experiment XXXIX., and proved that an extensive disturbance of the intestine lessens the amount of fluid in the injected coil. That this could be produced by merely a large number of ligatures, without the prolonged exposure and irritation of the intestine consequent on washing out and injecting several coils, the next experiment is designed to show.

*Experiment XLI.*—Cat, weighing 2·12 kilograms.

A loop about the middle of the small intestine was washed out and ligatured and injected with 1 c.c. of a 10 per cent. solution of a sulphate of soda, exactly as in Experiment XXXIII. Immediately afterwards four ligatures were rapidly tied round the cœcal end of the small intestine at a few inches apart from each other, and other four were similarly applied near the pyloric end. The exposure of the intestine was very short. Killed after five hours.

*AUTOPSY.*—The injected loop was 6 cm. in length, and contained 0·1 c.c. of a whitish viscid fluid. The portions of intestine included between the other ligatures were all empty.

In offering an explanation of the difference in the action of the salt on the large and on the small intestines, I have advanced the suggestion that during the first two hours or so after the injection of the salt it excites secretion, and that subsequently, owing to its having become diluted, it begins to be absorbed. Speaking more strictly, during the first period secretion is in excess of absorption, and during the second period absorption is in excess of secretion. For whenever a watery fluid is injected into the intestine we must believe that the two processes of absorption and secretion begin simultaneously to act, the excessive activity of the one over that of the other depending on the relation of the absorbability of the fluid to its excitatory power. That the salt solution injected, although scarcely increased in bulk at the end of five hours as in the preceding experiments, may, if interrupted at an earlier stage of

its action, show a decided increase, in conformity with my suggestion, is supported by Experiment XXXVI., where even a 5 per cent. solution, at the end of two hours, is increased in quantity in spite of the application of a large number of ligatures. *A priori*, we would expect that a 10 per cent. solution would show an increase. The result of the next experiment, in which all the conditions, except that of time, were exactly the same as in the preceding experiment, realised my expectation.

*Experiment XLII.*—Cat, female, weighing 1.47 kilograms.

All the conditions of loop, ligatures and injection, precisely as in the foregoing experiment; but animal killed at the end of *two hours*.

*AUTOPSY.*—The injected loop measured 6.5 cm. in length, and contained 3.7 c.c. of a colourless, tolerably viscid fluid, with large white flocculi. Alkaline reaction not strong, but distinct. The rest of the intestine was practically empty. The fluid possessed all the characters usually observed in such fluids—slight opalescence on acidifying and heating, good diastatic power, &c. The mucous membrane of the loop was pale, unless close by the seat of the ligatures, which is the condition ordinarily observed.

Secretion, then, does take place in the early stage of the action of the salt notwithstanding the application of a large number of ligatures. The only explanation that can be offered of the effect of the ligatures on the intestinal secretion is, that by the irritation of their presence they stimulate the absorptive power of the intestine, and, it may be, to a certain extent accelerate secretion, so that, at an earlier stage than without the irritation of ligaturing, absorption is able to counteract and eventually exceed secretion; or that the whole process of the action of the salt is hastened by an equal stimulation of absorption and secretion, the maximum dilution of the fluid being rapidly reached and its absorption quickly succeeding. In any case, there must be stimulation of the absorptive power of the intestine, and as that is accomplished through the action of ligatures placed at a considerable distance from the injected coil, we have every right to assume that there is here the manifestation of reflex stimulation of a set of intestinal nerves whose function it is to control absorption. Thus one more fact is added to demonstrate that absorption is under the influence of the nervous system, as has been satisfactorily proved, for

other portions of the body by the experiments of Brodie, Schiff, and Bernard, but most particularly by those of Goltz.

But, while the probable stimulation of absorption was the reflex effect of a number of ligatures, it was otherwise with the local or direct action of each ligature; for, as will be supported by the succeeding experiments, there is every reason to believe that the ligature, by the irritation and consequent congestion it produces of the immediately adjacent mucous membrane, gives rise to an increased secretion from the portion of the membrane so irritated. Such a zone of congestion, although in many instances extremely narrow, was invariably visible. It could not, however, of itself have been sufficient to cause an accumulation of secretion within a ligatured loop, or the control loops present in nearly all my experiments would not have been so completely empty as they almost without exception were. The central un-irritated portion of these loops must have been capable of absorbing the secretion as quickly as it was poured out. The result might naturally be expected to be otherwise when a solution of a purgative salt, or some other body capable of preventing the rapid absorption of the exuded fluid, was present in the loop. The secretion of fluid excited by the salt would then be augmented by that which was due to the mere local effect of the ligatures. To determine the truth of this hypothesis it was only necessary to perform some experiments in which the relative interference of the ligature secretion with the salt secretion should be reduced to a minimum by greatly extending the length of loop ligatured and equally increasing the amount of salt injected, whereby the salt secretion would be correspondingly increased without altering the amount of the ligature secretion. In some of the experiments already detailed I had observed the influence of different lengths of intestine on a given quantity of salt solution. Now, without altering the relation of the length of the intestine to the amount of the salt, I proposed to ascertain the effect of equally increasing both. The amount of secretion resulting from the injection of 1 c.c. of a 10 per cent. solution of the salt into a loop about 6 cm. long I had already observed, and it will be remembered that where it was unaccompanied by an excessive number of ligatures the secretion was abundant (Experiment XXXIII.



and others). Would the effect be proportionately the same if, say, the length of the coil was increased to 54 or 60 cm., and the amount of salt solution to 9 or 10 c.c., each being multiplied by 9 or 10? These experiments I undertook the more willingly as they offered the further prospect of a more complete and satisfactory examination of the secreted fluid than hitherto I had obtained owing to the small amount of the secretion.

As in none of the previous experiments had I been able to detect the escape of fluid from the injected loop into the adjacent controls, it appeared that the ligatures were in all cases applied sufficiently tightly to prevent such an escape, and to warrant the abandonment of the use of the two additional ligatures required for the controls.

The experiments were conducted in other respects in the manner previously described, the loop being well washed out with a warm  $\frac{3}{4}$  per cent. solution of common salt. Five ligatures were in each case necessary, unless where otherwise stated—two for the first demarcation of the loop, two to exclude the openings by which the loop had been washed, and one to shut off the puncture made at its one extremity by the injection-cannula.

*Experiment XLIII.*—Cat, male, weighing 3.51 kilogrammes. After the ligatures had been applied the loop was found to measure about 50 cm. long, and, accordingly, I injected 9 c.c. of a 10 per. cent. solution of sodic sulphate. Killed at the end of *five hours*.

*AUTOPSY.*—The fluid in the loop measured 5.6 c.c., and was colourless, opaque, and tolerably viscid, and alkaline in reaction. The opacity was caused by minute whitish flocculi. A few fragments of tape-worm, which the previous washing had failed to dislodge, were also present. That the ligatures were not so loosely applied as to permit the escape of some of the secretion from the loop was ascertained by attempting before opening the loop to press with considerable force some of the fluid through the ligatured ends of the loop, but not a drop flowed out. This precaution was used in every subsequent experiment. The injected loop was 51 cm. long, and was 12 cm. distant from the colon, and 63 cm. from the stomach.

The fluid after standing for a night became extremely tough, and altogether rather like a bronchitic sputum without the air-bubbles. Microscopically, the deposit consisted of a large number of nucleated, distinctly granular corpuscles, many of which were aggregated into mulberry-like groups; most of the corpuscles were globular, but a few were elongated, and almost perceptibly tailed. There was also a quantity of fine granular material resembling the debris of degenerated cells; some faintly fibrillar homogeneous matter was probably

mucin. A few granular columnar epithelial cells with large nuclei were also visible, as well as a very few nearly empty so-called chalice cells. There were no red corpuscles. I have not attempted to distinguish between mucous and lymph corpuscles, as physiologists are not generally agreed in what essential features the one differs from the other. According to the usual distinction most of the corpuscles appeared to be mucous.

The fluid contained a trace of albumen, and gave a distinct precipitate with acetic acid, insoluble in excess, indicating the presence of mucin. The whole of the fluid was carefully collected from the microscopic slide, test-tube, and loop, the last of which was infused several times in water, and the infusion added to the fluid, and the mixture analysed to ascertain the quantity of sulphates present. For this purpose it was evaporated to dryness, burnt with a little dried carbonate of soda, and the ash dissolved in hydrochloric acid and water, and the sulphuric acid estimated in the filtrate by the usual method. The quantity of sulphuric acid recovered, estimated as  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , was 0.041 gramme, or only a fraction of what had been injected.

The result of this experiment was remarkable. The salt solution, instead of increasing in amount, as might have been expected from a knowledge of the action of the salt as exhibited in Experiment XXXIII., has diminished.

Was this diminution due to causes similar to those which lessened the fluid in the experiments with a large number of ligatures? Had the exposure of the large loop of intestine during the time occupied in washing it irritated it much in the same manner as the excessive number of ligatures, so that its absorbing or secreting activity was affected? Another experiment, in which all irritation was as much as possible avoided, appeared therefore desirable.

*Experiment XLIV.*—Cat, male, weighing 3.40 kilogrammes. Small intestine was exposed, and about 50 cm. quickly measured and ligatured. The loop was quite collapsed and empty, and was neither stripped nor washed out. Into it were injected 10 c.c. of a 10 per cent. solution of sulphate of soda, heated to 35° C., and, as before, the puncture was ligatured off. Altogether three ligatures were applied, and the time of exposure of the intestine did not exceed two minutes. Killed *five hours* afterwards.

*AUTOPSY.*—The loop contained 6 c.c. of a somewhat viscid, colourless fluid, transparent where unmixed with yellowish-white, extremely tough flakes, which constituted the larger part of the liquid. Reaction, a'kaline. The exact length of the loop as measured after death was 52 cm. The mucous membrane was distinctly paler than in the

remainder of the small intestine, which was collapsed and empty in its whole extent. The total length of the small intestine was 153 cm., and the loop was 7 cm. from the cæcum. The stomach contained 2.5 c.c. of a frothy yellowish fluid, acid in reaction. In the colon was a quantity of firm fæces, but no fluid.

The fluid obtained from the injected loop along with several infusions of the latter yielded 0.101 gramme of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , reckoned from the sulphuric acid.

This experiment in its result was an exact confirmation of Experiment XLIII., both showing that in a sufficiently long loop a 10 per cent. solution of sulphate of soda is diminished rather than increased during a five hours' stay in the intestine. It were, however, possible that in the earlier part of the action of the salt there was an actual increase of the secretion, which had diluted the salt sufficiently to permit of its absorption before the expiry of five hours, as had already been observed with regard to injected loops influenced by the presence of many ligatures. The next experiment shows that there was no such increase of the fluid during the earlier part of the action of the salt.

*Experiment XLV.*—Cat, female, weighing 2.29 kilogrammes. Injected into a loop of the small intestine, washed out, and ligatured as in Experiment XLIII., 9 c.c. of a 10 per cent. solution of sulphate of soda. The cat vomited a very little about one hour and a quarter after the operation. Killed in *two hours*.

*AUTOPSY.*—On exposing the intestines *immediately* after death, the following appearances were noted, and, although specially mentioned here, were common to all the experiments of the same kind. Peristalsis was rather less active in the injected than in the uninjected part of the intestine. The former was distinctly paler than the latter, and its blood-vessels were less distended and less prominent. No part of the intestine contracted very readily on being lightly stimulated, and still less so the injected loop. After removal from the abdomen by detachment from the mesentery, the excitability of the muscular walls of the injected loop was little, if at all, increased. On the other hand, the remainder of the intestine was sensible to very slight stimulation, contracting firmly and often permanently along a much greater part of its length than that irritated.

The injected loop, which measured 44 cm., contained 9.9 c.c. of a partly limpid, partly viscid fluid, colourless and transparent, unless where mixed with some faintly yellowish-white flocculi. Reaction, alkaline.

Microscopically, the deposit presented the same characters as in Experiment XLIII., but contained rather more mucous corpuscles.

A portion of the fluid boiled did not show any visible coagulum. Treated with excess of acetic acid the unboiled fluid deposited a moderate quantity of mucin, the filtrate from which, mixed with a drop of a solution of ferrocyanide of potassium, became very faintly opalescent, indicating the presence of a mere trace of albumen. This was confirmed by other delicate proteid reactions.

The fluid digested starch readily, and inverted cane-sugar. Collecting carefully all the various portions of the fluid and adding the infusion of the loop, and allowing for the sulphate of copper used in testing for albumen and sugar, the whole mixture yielded 0.322 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , estimated from the sulphuric acid recovered.

These three experiments, presenting results in perfect concord, more than prove what I expected, that the increase of the salt solution witnessed in a short intestinal loop is in large part due to the local irritation of the ligatures. For when a much longer loop was injected, with the ligatures so widely apart that the secretion they excited could exert but little appreciable influence on the large mass of injected fluid, then a 10 per cent. solution of sulphate of soda was found not to have increased in volume, whether examined two hours or five hours after injection. Had it increased to the same extent as in Experiment XXXIII., where 1 c.c. became 3.45 c.c., the 9 c.c. ought to have swelled to 30 c.c.

Had these last experiments been made previous to those which form the earlier part of this series, I would doubtless have been satisfied with the support and proof they offered of the theory I sought to establish in the first series of experiments—that the salt does not excite secretion, but merely prevents the absorption of the fluid in which it is dissolved. And even taking into consideration all the experiments of this series I have as yet recorded, they do not present much in opposition to this theory but what might be roughly explained by the conditions of the experiments. But in attempting a precise explanation of many of the phenomena observed, I have assumed that the salt excites intestinal secretion; and this is borne out, even in those cases where the injected fluid undergoes no increase, by an examination of the chemical characters and digestive properties of the fluid. If the bulk of the fluid is not increased, it is because absorption has proceeded as rapidly as secretion. The small quantity of the salt recovered from the injected loop (Experi-

ments XLIII., XLIV., and XLV.), the viscosity of the fluid, and its action on starch and cane-sugar, all prove that the salt solution has undergone considerable changes, which can only be accounted for by a free secretion, obscured in certain cases by an equally free absorption.

Proceeding to make a practical application of the results of these experiments, are we to believe they fairly represent what occurs when an ordinary dose of the purgative salt is administered by the mouth? Am I right in assuming that the salt is diluted to at least a 10 per cent. solution before it passes into the intestine; and does the salt solution, diluted to that strength by the alimentary fluids, undergo no increase in the canal, although exciting secretion; or is the proportion of the salt solution to the length of intestine acted on too small in my experiments to represent what happens when the salt reaches the intestine after administration in the usual manner; and will an alteration of this proportion affect the amount of the fluid within the canal? Further, if the salt can reach the intestine in a more concentrated solution than 10 per cent., may the action of the concentrated salt be different from that of the diluted salt? All other observers employed a 20 per cent. solution for injection into the intestine, and obtained in many cases a very large increase of fluid.

In answer to the first of these questions, as to the strength of the salt solution when it passes into the intestines from the stomach, and as to the proportion of it to the length of the intestines acted on, I think we have every right to assume that the salt is very rarely stronger than 10 per cent. when it passes through the pylorus. A dose of the salt, say an ounce, is generally taken, dissolved in almost a tumblerful or 10 ounces of water, including that which is customarily drank immediately afterwards; and even if it be taken more concentrated, it is very likely to meet with sufficient fluid in the stomach to dilute it to at least a 10 per cent. solution. So there can be no objection to the strength of the solution I have employed. The proportion of the salt solution injected to the length of intestine acted on is not, however, so free from fault. And an alteration in this proportion may very materially affect the bulk of the fluid, as Experiments XXXVIII. and XXXIX. have proved; although

in these the coils were so short that the difference observed may be largely attributable to the effect of the ligatures. There a given quantity of a 10 per cent. solution of the salt showed much the larger increase in the longer of two loops, which is equivalent to stating that if the loops be made of equal length, and more of the salt solution be injected into the one than into the other, the secreted fluid will be relatively much larger in the former than in the latter. It is probable, therefore, that had I injected more than 9 or 10 c.c. into a loop 50 to 60 c.c., as in the last three experiments, the solution, instead of showing no increase, might have been considerably augmented. But it is yet to be decided whether the proportion of the salt solution to the length of intestine did not represent what actually occurs when the salt is administered *per os*. In the cat, the animal used in the foregoing experiments, the small intestine varies, as I have frequently ascertained by measurement, from 110 or 120 to 170 cm. Now, as the salt solution will pass through the pylorus comparatively slowly, the first portion of the salt will almost have reached the cœcum before the last portion has left the stomach, so that the 5 grammes of salt, the purgative dose for a cat, will be distributed over the whole length of the small intestine. There will thus be 1 gramme for every 22 to 34 cm. of intestine. But, if it happens that a portion of the salt has passed into the colon before the last of it has left the stomach, then this proportion may be diminished to 1 gramme for every 40 or even 60 cm. In my experiments the proportion was 1 to 60, and this in some cases might represent what takes place. But in all likelihood the proportion is greater—1 to 30, or so. The proportion I used was arbitrarily chosen, and without regard to the point now under discussion. If the proportion be increased to 1 to 30 (or 2 to 60), will the effect of the salt solution exhibit the same difference in the large loops as it did in the short loops of Exp. XXXVIII. and XXXIX.? For this is the important point. Experiment answers in the affirmative.

*Experiment XLVI.*—Cat, male, weighing 2·86 kilograms. Loop of small intestine washed out and ligatured in the usual manner, and injected with 20 c.c. of a 10 per cent. solution of sulphate of soda. After death the loop was found to measure 54 cm. long. The animal was killed in *five hours*.

**AUTOPSY.**—The loop contained 34 c.c. of a fluid possessing much the same characters as the fluids previously obtained,—colourless, somewhat viscid, containing only a trace of albumen, and capable of digesting starch and cane-sugar.

In Series D. of my experiments, where I had occasion to inject the whole length of the small intestine, ligatured at both its extremities, but excluding the pancreatic and bile ducts, with 5 grammes of the salt in the form of a 10 per cent. solution, I found that the 50 c.c. injected generally increased within a short time to about 100 c.c. There is, therefore, hardly any doubt that, if the proportion of the salt solution to the length of the intestinal loop be greater than 1 to 60, there will be an increase in the bulk of the fluid, and this is probably what occurs when the salt is given by the mouth. So that in this point of proportion many of my experiments are open to objection, without, however, greatly impairing their interest and physiological value.

We have yet to discuss the effect of the salt reaching the intestine in a possibly more concentrated solution than 10 per cent. This I have already stated as being highly improbable. But, apart from its probability, the investigation of this point will be of interest as forming the link between my experiments and the experiments of Colin, Moreau, Vulpian, Lauder Brunton, and the others who injected a 20 per cent. solution of the purgative salt into the intestine. Will a given weight of the salt injected into a given length of intestine affect differently the resultant quantity of fluid according as the salt solution is concentrated or dilute? Although this has already been answered in the affirmative, in so far as short loops are concerned, by Experiment XXXVII., it was desirable to investigate the matter further.

*Experiment XLVII.*—Cat, male, weighing 2.03 kilograms. Injected into a loop of the small intestine (which was after death ascertained to measure 52 cm., and was situated 9 cm. from the cœcum, and 67 cm. from the pylorus),  $4\frac{1}{2}$  c.c. of a 20 per cent. solution of sulphate of soda, containing, therefore, as much salt as 9 c.c. of a 10 per cent. solution, the quantity used in the preceding experiments. The loop was previously washed out as usual. A little vomiting occurred during the recovery of the animal from the anæsthetic condition. Killed at the end of *three hours*. During this interval it evacuated a small





ntly for six hours afterwards it gave no indication of inversion  
ng occurred. Next day, or nearly twenty hours after mixture, it  
ed Fehling at once. Allowed to stand for another day, 12·3 c.c.  
ed to decolorise 20 c.c. of Pavy's solution, so that the whole  
contained 0·081 grms. of maltose; and this, therefore, represents  
versive power of 5 c.c. of the intestinal fluid.

f of the original fluid, along with half of the infusion of the  
ielded 0·311 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$  (as reckoned from the  
ic acid), or for the whole fluid 0·622 grammes.

nucous membrane of the injected loop was perfectly pale, and  
l not the slightest signs of congestion in any part of its length  
the immediate neighbourhood of the ligatures.

s experiment differed so remarkably from the preceding  
he large amount of secretion excited by the salt, it was  
sirable to obtain confirmation of its correctness.

of XLVIII.—Cat, female, weighing 1·36 kilogrms., very  
ed  $4\frac{1}{2}$  c.c. of a 20 per cent. solution of sulphate of soda  
the small intestine, which was ascertained after death to  
g and 7 cm. from the cæcum. The intestine was not  
at the same number of ligatures was applied as if it had  
to render the conditions similar to those of preceding  
Immediately after recovering from anæsthesia it vomited  
some time kept mewling rather loudly as if suffering  
the first, and, indeed, the only occasion, on which I  
ing evidence of suffering pain after the operation  
imal usually rested quietly and placidly. Killed at  
78.

ected loop contained 8 c.c. of a yellowish-white  
with a large quantity of flaky mucus; reaction,  
ght hæmorrhage had taken place into the peri-  
neighbourhood of one of the ligatures. The  
y the additional ligatures were empty; and the  
the injected loop were found to be sufficiently  
pe of fluid.

efore, presented a widely different result  
periment. The saline solution, as in  
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l seemed to suffer pain, and it is possible  
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quantity of firm fæces, the only occasion on which I have observed this occurring in the whole of these experiments.

**AUTOPSY.**—The loop was tolerably distended with 36 c.c. of a colourless, opalescent, somewhat viscid fluid, mixed with white mucous flakes. Reaction, alkaline; 0.318 c.c. of the pharmacopœial standard solution of oxalic acid<sup>1</sup> was required to neutralise 10 c.c. of it. The acid solution actually used was ten times more dilute than the pharmacopœial.

Examined for albumen, the fluid gave a slight opacity with excess of acetic acid and a drop or two of ferrocyanide of potassium. Compared with an equal quantity of pure saliva in a similar-sized test-tube, and treated with the same reagents, the opalescence of the former was a shade more distinct than that of the latter. I made this comparison in order to ascertain by a simple method whether the quantity of albumen present in the intestinal fluid exceeds that found in other alimentary secretions. A large quantity of albumen would have given support to Vulpian's view,<sup>2</sup> that the secretion is of an inflammatory character. Heated with sulphate of copper and caustic potash, a very faint violet was produced, which was not deeper than that given by saliva with these reagents. No blackening with acetate of lead, and therefore no sulphides. (It has been stated by some authors<sup>3</sup> that the purgative sulphates are partly decomposed in the canal, forming sulphides, which by irritating the intestinal mucous membrane excite secretion). Excess of chlorine water added to the intestinal fluid did not, even after several hours, produce any change of colour, as it is stated to do with pancreatic juice.<sup>4</sup>

In order to ascertain quantitatively the diastatic power of the fluid, 5 c.c. of it were mixed with 100 c.c. of a 1 per cent. solution of pure starch. Both fluids were previously tested for glucose or maltose, and found to contain none. The mixture was placed in an oven at a temperature of about 50° C., and tested at intervals of ten minutes for maltose. Fehling's solution was employed for this purpose, and within thirty minutes the first evidence of the presence of maltose was obtained. The mixture was then digested for forty-eight hours, by which time the action of the ferment might be supposed to be exhausted. It now abundantly reduced Fehling, and 10.5 c.c. of it decolorised 20 c.c. of Pavy's modification<sup>5</sup> of Fehling's solution, equivalent to 0.135 grammes of maltose. The 5 c.c. of the original intestinal fluid therefore contained sufficient ferment to form 0.128 gramme of maltose from starch.

The invertive power of the fluid was similarly measured. 5 c.c. of the intestinal fluid were mixed with 1 gramme of cane-sugar dissolved in 100 c.c. of water, and placed in the digesting oven. Tested fre-

<sup>1</sup> 100 c.c. of the standard solution of oxalic acid of the British Pharmacopœia contain 6.3 grms. of the crystalline acid.

<sup>2</sup> Page 249 of the present volume of this Journal.

<sup>3</sup> Buchheim, *Arzneimittellehre*, 3 Auflage, S. 133.

<sup>4</sup> Hoppe-Seyler's *Physiologische Chemie*, S. 258.

<sup>5</sup> A mixture of Fehling's solution with a large quantity of strong ammonia.

quently for six hours afterwards it gave no indication of inversion having occurred. Next day, or nearly twenty hours after mixture, it reduced Fehling at once. Allowed to stand for another day, 12·3 c.c. sufficed to decolorise 20 c.c. of Pavy's solution, so that the whole fluid contained 0·081 grms. of maltose; and this, therefore, represents the inversive power of 5 c.c. of the intestinal fluid.

Half of the original fluid, along with half of the infusion of the loop, yielded 0·311 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$  (as reckoned from the sulphuric acid), or for the whole fluid 0·622 grammes.

The mucous membrane of the injected loop was perfectly pale, and exhibited not the slightest signs of congestion in any part of its length unless in the immediate neighbourhood of the ligatures.

As this experiment differed so remarkably from the preceding three in the large amount of secretion excited by the salt, it was thought desirable to obtain confirmation of its correctness.

*Experiment XLVIII.*—Cat, female, weighing 1·36 kilograms, very lean. Injected  $4\frac{1}{2}$  c.c. of a 20 per cent. solution of sulphate of soda into a loop of the small intestine, which was ascertained after death to be 60 cm. long and 7 cm. from the cæcum. The intestine was not washed out, but the same number of ligatures was applied as if it had been, in order to render the conditions similar to those of preceding experiments. Immediately after recovering from anaesthesia it vomited a little, and for some time kept mewing rather loudly as if suffering pain. This was the first, and, indeed, the only occasion, on which I observed a cat giving evidence of suffering pain after the operation practised, as the animal usually rested quietly and placidly. Killed at the end of five hours.

*AUTOPSY.*—The injected loop contained 8 c.c. of a yellowish-white viscid fluid, mixed with a large quantity of flaky mucus; reaction, alkaline. A very slight hæmorrhage had taken place into the peritoneal cavity from the neighbourhood of one of the ligatures. The control loops formed by the additional ligatures were empty; and the ligatures at the ends of the injected loop were found to be sufficiently tight to prevent any escape of fluid.

This experiment, therefore, presented a widely different result from the preceding experiment. The saline solution, as in experiments where a 10 per cent. solution was employed, had scarcely increased in bulk, and certainly did not exhibit the remarkable increase observed in Experiment XLVII. The result of the one or of the other experiment must be incorrect, owing to bad conditions. These are most apparent in Experiment XLVIII., where the animal seemed to suffer pain, and it is possible that reflex inhibition of secretion or stimulation of absorption

may have been induced by the circumstance which occasioned the pain—perhaps, the inclusion of a mesenteric nerve in the intestinal ligatures, or of a cutaneous nerve in the abdominal sutures. I therefore inferred that Experiment XLVII. presented the more reliable result. But this required confirmation, and necessitated the experiment which follows.

*Experiment XLIX.*—Cat, male, weighing 2·21 kilogrammes. Injected into a loop of the ileum, without previous washing-out,  $4\frac{1}{2}$  c.c. of a 20 per cent. solution of sulphate of soda. The usual number of ligatures were placed round the intestine. Killed after *five hours*.

*AUTOPSY.*—The loop contained 54 c.c. of nearly colourless fluid, mixed with a very little brownish shreddy material, probably a residue of the food, also with a quantity of whitish flocculi. Reaction was alkaline, 10 c.c., requiring 0·444 c.c. of the standard solution of oxalic acid for neutralisation.

The fluid became distinctly opaque on the addition of acetic acid and ferrocyanide of potassium; and compared with saliva, by placing it in an equal-sized tube and diluting it until a similar degree of opacity was reached, I believed it to contain about twice as much albumen as that secretion. The excess of albumen was easily accounted for by the presence of a little incompletely digested food in the injected loop. Other reagents for albumen gave the same result. 5 c.c. of it mixed with 100 c.c. of a 1 per cent. solution of pure starch gave the first indication of the presence of maltose in about fifteen minutes, and, after standing for forty-eight hours, yielded 0·4890 grammes of maltose. Other 5 c.c. mixed with 100 c.c. of a 1 per cent. solution of cane-sugar digested it slowly, and gave at the end of forty-eight hours 0·093 gramme of invert sugar. Half of the original fluid with half of the infusion of the loop yielded 0·412 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , or, for the whole fluid, 0·824 grammes.

The injected-loop measured 53 cm. long, and was emptied before injection by gentle stripping, washing out having been omitted, as I wished to complete the operation as quickly as possible in order to avoid prolonged exposure and irritation of the intestine.

The mucous membrane of the loop was quite pale, and was covered with a layer of viscid mucus mixed with epithelial debris.

The result of this experiment is in harmony with that of Experiment XLVII. In both there was a very decided increase of fluid after the injection of a 20 per cent. solution of the salt, in the proportion of 1 gramme of the salt to 60 cm. of intestine, or the same as used in Experiments XLIII., XLIV., and XLV.; and we may safely conclude that the result of Experiment XLVIII. was exceptional.

Before proceeding to compare the effects of a dilute and a

concentrated solution of the salt as brought out in these experiments, the digestive capabilities of the intestinal fluids obtained will be alluded to. The diastatic power of the secretion was in no case great. The more energetic action of the fluid of Experiment XLIX. was, doubtless, due to the presence of a small quantity of pancreatic juice, from the loop not having been washed out. The action of the secretion on cane-sugar was still less marked. The exact extent of the action is in each case recorded in the protocol. Some additional experiments were made to ascertain in how far the secretion was able to digest albumen and fat. Placed in a digesting oven with a small piece of fibrin, the fluid gave no evidence of peptones, except in the case of Experiment XLIX., and then only a trace, even after several hours, although the fibrin was generally in part dissolved. The secretion also failed to digest or emulsify fats in the form of pure acid-free cod liver oil, neither when first mixed with it, nor after standing for nearly twelve hours.

Passing now to a comparison of Experiments XLIV., XLV., and XLVI. with XLVII. and XLIX., we are confronted with the surprising fact that a given quantity of sulphate of soda, injected into a certain length of intestine, will, if dissolved as a 10 per cent. solution, cause little or no alteration in the volume of fluid within the intestinal loop; but, if the same amount of the salt be injected in the form of a 20 per cent. solution, or twice as concentrated as the other, and into the same length of intestine, it will provoke a large flow of secretion, far beyond what is necessary to bring the salt to the same state of dilution as in the former case, and where we might have expected its increase to be arrested. This difference of effect between the strong and weak solutions is not in accordance with what was observed in Series A. of experiments to follow their introduction by the mouth. There it was remarked that the more concentrated the salt, the less powerful, or at least the more delayed, was its action. It is not difficult to understand how this may be. For, excluding these experiments where water was withheld from the diet of the animals for some days previously, a concentrated solution of the salt would, when administered *per os*, become quickly diluted in the stomach or upper part of the intestine, either by the fluids present or

by the secretion excited, or by both, and passing into the general length of the intestine, would then merely exert the action of a weaker solution under this disadvantage, that the blood, having already in the upper part of the alimentary canal supplied it with some of its fluid, and thus become more concentrated, would less freely permit of secretion in the intestine generally, and thus delay purgation. It is otherwise when the concentrated salt is kept for some time in contact with the intestine in a ligatured loop. The solution, although diluted by secretion, is acting on a mucous membrane, altered by the application of an originally concentrated salt. What the nature of this alteration is, cannot be easily determined. It is not a strong irritation and inflammatory condition of the intestine, or the mucous membrane would have been congested, which it on no occasion was; and the secretion ought to have contained excess of albumen, which it never did. The concentrated salt evidently produces some more subtle and less obvious change, whereby the secreting power of the intestine is increased or its absorptive power is diminished; for either the one or the other, or a combination of them, will suffice to increase the fluid. Disregarding any impression which the salt may make upon the secretory nervous mechanism of the intestine, and which may be maintained for some time after the cause has disappeared or the salt become diluted, it is probable that the concentrated salt affects the absorptive more than the secretory power of the mucous membrane. For we cannot well conceive of the salt producing in the secretory cells of the Lieberkühnian follicles any change, other than inflammatory irritation, which would not pass away with the dilution of the salt. On the other hand, there are several facts which give countenance to a supposed diminution of the absorptive power of the membrane. Absorption, without doubt, is for the most part effected through the agency of the cylindrical epithelium covering the villi of the intestine; and any injury to this must impede absorption. Now, in microscopical sections of the mucous membrane of the various injected loops, I have observed that chalice or goblet-shaped cells were unusually abundant where the solution injected was 20 per cent. The chalice cells were evidently formed from the cylindrical cells by the action of the salt; and we may

assume, without being at variance with the most recent physiological knowledge, that the former cells have a much lower absorptive power than the latter. A 10 per cent. solution did not produce nearly so many chalice cells.

That the absorptive power of the mucous membrane of the intestine was lessened by a strong solution of the salt, was further supported by the fact, that after the action of such a solution, much more of the salt was recovered from the contents of the loop than after the action of a weaker solution. The salt recovered in the last six experiments clearly shows this. A thick, viscid, layer of mucus was generally observed coating the surface of the mucous membrane after the action of the concentrated salt, and this also may have offered a hindrance to absorption.

An examination of these various points has led me, therefore, to conclude that the difference between the action of a 20 per cent. solution of sulphate of soda and of a 10 per cent. is due more to impeded absorption than to accelerated secretion. There is yet some experimental evidence to adduce in favour of this view, and which I shall now present.

Moreau<sup>1</sup> recently communicated to the Académie de Médecine the results of a number of experiments which went to prove that sulphate of soda or sulphate of magnesia when injected into the intestine is not absorbed. This fact he urged as of great importance in showing the fallacy of the belief, entertained by Liebig, Rabuteau, and others, that osmosis plays a part, if not the sole part, in the production of secretion by the salt. It seems hardly necessary to combat Moreau's statement with further experiment than is offered in Series A. of this paper, where a large proportion of the salt was in several instances recovered from the urine. More than this I should not have done, had I not, previous to having read Moreau's communication, made two experiments by a method like his, for the purpose of ascertaining whether absorption took place more rapidly from a loop containing a 20 per cent. solution of the salt than from one containing a 10 per cent. solution. Moreau injected a 20 per cent. solution of the salt into the ligatured intestinal loop of a dog, and a few minutes afterwards injected into the same loop a solution

<sup>1</sup> Moreau, *Bulletin de l'académie de médecine*, 2me série, t. viii. 1879, p. 357 ; *ibid.* p. 367.

of ferrocyanide of potassium. In no instance, and his experiments were numerous, could he discover the ferrocyanide in the urine with the usual reagents—perchloride of iron or sulphate of copper. He did not take the precaution of concentrating the urine by evaporation before testing, which would have rendered his analysis more satisfactory. Absorption, as I have already observed, is doubtless impeded, but is not in abeyance; and the ferrocyanide, passing very slowly into the blood and reaching the urine in still more diminished amount, may not have been present in that secretion in sufficient quantity to yield a perceptible colour with the reagents. Moreau draws no distinction between the action of a concentrated salt and a dilute salt, and evidently believes that the action of a 20 per cent. solution injected directly into the intestine represents what happens when it is given by the mouth. Even apart from the experiments of Series A., I did not doubt the absorption of a 10 per cent. solution, if for no other reason than that so little of the injected salt could be recovered from the loop. Where a 20 per cent. solution was injected, the salt unrecovered was so small that it might readily have been imbibed by the tissues of the intestine without actually passing into the circulation, so that I had reason to doubt its absorption, conceiving that the mucous membrane might be so altered by the strong solution as to be incapable of absorbing. This led to the two following experiments being devised. They happily, at the same time, offer a complete refutation of Moreau's *prima facie* unlikely conclusions. My method chanced to be similar to Moreau's; but, instead of the ferrocyanide, I used strychnia, a poison which, if absorbed by the blood, will very quickly manifest its effects without its being necessary to detect it chemically.

Falck<sup>1</sup> states that 0·75 milligrm. per kilogrm. of the animal's weight is the smallest lethal dose for a cat. I injected rather more than double that proportion in each of the following experiments:—

*Experiment L.*—Cat, male, weighing 2·75 kilogrms. Exposed the small intestine and injected into a loop of it, ligatured as usual, 10 c.c. of a 20 per cent. solution of sulphate of soda. The intestine was returned within the abdomen, and the edges of the parietal incision

<sup>1</sup> Falck, *Roszbach u. Nothnagel's Arzneimittellehre*, 1878, S. 699.



held together, so as to prevent exposure of the gut for the next fifteen minutes, when it was again withdrawn,—the salt, meanwhile, having had sufficient time to produce its action on the mucous membrane of the loop. 1 c.c. of a solution of strychnia, containing 5·16 milligrms. of the alkaloid, was then injected into the centre of the same loop, the cannula of the syringe being first directed towards one extremity, and afterwards towards the other extremity of the loop, so as to ensure mixture of the strychnia with the salt solution.

In twenty-one minutes after the injection of the strychnia, the first symptoms of its action became apparent in the form of slight twitchings of individual muscles. The animal was released from the holder immediately after the completion of the operation, and was tolerably conscious when placed on the floor, so that the anæsthetic did not retard the advent of the toxic symptoms. In nine minutes more there were well-marked tetanic spasms of the whole body, throwing it into the opisthotonic position. These continued with very short remissions until the respiratory muscles became involved and breathing was arrested. Three minutes later, or thirty-nine minutes after the injection of the alkaloid, the heart's action ceased. During this period so much of the strychnia must have been absorbed as was necessary to cause death. According to Falck's statement of the lethal dose, this quantity must have corresponded to about one-half of what was injected.

**AUTOPSY.**—The loop was 52 cm. long, 12 cm. from the cæcum, and contained 36 c.c. of a yellowish-white viscid fluid. Half of the fluid, with half of the infusion of the loop, yielded 0·691 gramme of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , and therefore from the whole fluid there was recovered 1·382 grammes out of 2 grammes injected.

Although the fluid was not well suited for ascertaining the diastatic power of the secretion owing to the intestine not having been previously washed out, yet 5 c.c. of it were mixed with 100 c.c. of a 1 per cent. solution of starch, and digested in an oven. Seven hours afterwards maltose first made its appearance. For the sake of comparison 52 cm. of the uninjected portion of the small intestine, washed and cut into very small shreds, were infused in 36 c.c. of distilled water. After macerating for one day, 5 c.c. of the infusion were mixed with 100 c.c. of a 1 per cent. solution of starch. Maltose appeared in from five to six hours, but not distinctly until near the end of the eighth hour. Both the secretion and the infusion were without action on cane-sugar. This is to be attributed to certain conditions which I hope to incorporate in another paper. It is interesting to have observed that the diastatic power of the secreted fluid was not stronger than a corresponding infusion of a fresh portion of the intestine.

The mucous membrane of the injected loop was, as in previous experiments, perfectly pale.

**Experiment LI.**—Cat, male, weighing 2·77 kilogrms—of the same sex, and nearly of the same weight and size as that used in the preceding experiment. The operation was in all respects the same as the other with the important exception that 20 c.c. of a 10 per cent.

solution of sulphate of soda were injected instead of 10 c.c. of a 20 per cent. solution—therefore, the same amount of salt but more concentrated. Fifteen minutes later the same quantity of the strychnia solution was injected as in the last experiment. Within two minutes some slight general twitchings of the muscles were observable. In six minutes more the animal gradually assumed the opisthotonic position, and two minutes afterwards there was a severe general tetanic spasm. The spasm was renewed within a minute, and continued without relaxation until the animal died, thirteen minutes and a half after the injection of the strychnia.

**AUTOPSY.**—In the injected loop, which measured 56 cm. in length, and was situated 8 cm. from the cœcum, were 26 c.c. of a colourless limpid fluid mixed with a small quantity of shreddy undigested remnants of food. As 21 c.c. had been injected, including the 1 c.c. of the strychnia solution, there was an increase of 5 c.c., which is to be explained by the amount of the 10 per cent. solution to the length of intestine injected exceeding the proportion in which I have found no increase to take place.

These two experiments conclusively prove, if such proof were necessary, that, in opposition to the statement of Moreau, the small intestine retains its power of absorption even when a 20 per cent. solution of a purgative salt has been injected. They also show, what previous experiments have led me to believe, that the absorptive power of the intestine is less injured by a weak solution of the salt than by a strong solution.

It were possible, as I have hinted, that some viscid or other character of the fluid secreted under the stimulus of the concentrated salt might in part, if not wholly, account for the slow absorption of the secretion and its consequent accumulation within the intestine. For the secretion obtained by the action of the concentrated salt was always more viscid than when the diluted salt was employed; and a layer of thick tough mucus was generally observed lining the mucous membrane of the loop into which the former had been injected. To settle this point, yet another experiment was made, in which a 20 per cent. solution of the salt was removed from the intestine after it had been allowed to act for an hour. The injected loop was then carefully washed out, and a portion of it injected with a fresh but weak saline solution, the remainder receiving nothing. For the sake of comparison, an unused part of the intestine was injected with the same weak saline solution. The result gave further confirmation of the conclusion to which the previous

experiments led, that the absorptive power of the intestine was largely impaired by the action of the concentrated salt.

*Experiment LII.*—Cat, female, weighing 2·05 kilograms. Small intestine exposed, and 64 cm. measured off and ligatured, and washed out with a warm  $\frac{3}{4}$  per cent. solution of chloride of sodium. The openings formed for the purpose of washing were ligatured off, and the loop thus prepared was divided into two unequal parts by another ligature, the one part being, as nearly as possible, twice the length of the other. Into the longer were injected 10 c.c. of a 20 per cent. solution of sulphate of soda, but nothing into the shorter. The intestine was returned within the abdomen, and the parietal wound was closed with a clamp for an hour, during which the animal was kept lightly anaesthetised. At the end of the hour the intestine was again exposed, and the injected loop was observed to be well distended with fluid, which was now removed by a small incision in one extremity of the loop. The fluid measured 26 c.c., and was clear, colourless, and distinctly viscid, becoming much more viscid after standing for an hour or two; it contained a large amount of opaque, viscid, flaky material. The uninjected portion of the loop was perfectly empty. The injected loop was next carefully washed out with a warm 1 per cent. solution of sulphate of soda, and emptied, and the opening was ligatured off, and the loop divided into two equal parts by another ligature. There were, therefore, altogether three loops of nearly equal length on the intestine. Into one of these nothing had been as yet injected, whilst the other had suffered from the action of a 20 per cent. solution of sodic sulphate. Into the first of these, and into one of the other two, were now injected  $7\frac{1}{2}$  c.c. of a 1 per cent. solution of the salt. The third was not interfered with. The cat was killed one hour afterwards.

*AUTOPSY.*—The twice injected loop contained 7·8 c.c. of a colourless fluid, mixed with some white viscid flocculi, and was alkaline in reaction. The other, but only once, injected loop contained 1 c.c. of a viscid fluid. The third loop, forming the half of the longer loop previously injected with the strong solution of the salt, and afterwards emptied, contained not a drop of fluid.

From this experiment we learn that after the removal of a strong solution of sulphate of soda from a loop into which an hour previously it had been injected, fluid no longer accumulates within the loop. But if a very weak solution of the salt be injected into such a loop it will not disappear as it does from a fresh loop. The latter fact shows that the absorptive power of the mucous membrane has been impaired by the action of the strong solution of the salt; the former points to the viscid absorption-hindering character of the secretion excited by the salt as being a factor in the increase of the contents of the

loop. For, if instead of a weak solution of the salt, had the fluid secreted by the concentrated salt been allowed to remain in the loop, previous experiments teach us that the fluid would have continued to increase; and this was not due to the excessive excito-secretory power of the salt present, as by the time of its removal from the loop it was so diluted (10 c.c. to 26 c.c.) that its strength was even less than 10 per cent., a strength which we know in the proportion injected does not cause accumulation of fluid within the loop in which it is placed.

A careful examination was made of the digestive properties of the 26 c.c. of intestinal fluid obtained at the end of the first stage of the last experiment. This was the more exactly done, as at the time I was making a number of experiments to ascertain the comparative digestive powers of several animal fluids and secretions, in order to have some idea of the relative importance of the digestive power of the intestinal juice. As in testing its diastatic activity I formerly observed within what time maltose first made its appearance when the intestinal fluid was mixed with a 1 per cent. solution of starch, I adopted the same method in the present examination, altering, however, the proportion of the fluid to the starch solution, from 5 to 100 to 5 to 10. For I had observed that the greater the amount of the starch solution, and, especially, the stronger the solution, the later appeared the maltose; and working with weak diastatic fluids, as intestinal juice, if the starch solution were strong enough, say 5 to 10 per cent., maltose never appeared, or at least was not detectable by means of Fehling's solution. This may account for the variety of opinion existing as to the diastatic power of the intestinal juice. 5 c.c. of the intestinal secretion of the last experiment were, therefore, mixed with 10 c.c. of a 1 per cent. solution of starch, and placed in a digesting oven. Within half an hour maltose appeared, and by the end of five hours the mixture ceased to colour iodine either blue or red, showing that both starch and erythrodextrin, the intermediate product in the digestion of starch, had completely disappeared.

Other 5 c.c. were mixed with 10 c.c. of a 1 per cent. solution of cane-sugar. In one hour there was a trace of invert-sugar, which after some hours became more, but never very, distinct.

To yet other 5 c.c. were added 0.5 gramme of fresh fibrin. In

two hours there was no perceptible change. Four hours later it was completely disintegrated, and for the most part dissolved. The fluid gave a very feeble peptone reaction, but a considerable deposit with the albumen precipitants, so that, although the fibrin had been dissolved, not much of it had been peptonised or truly digested. The alkali of the intestinal secretion had, no doubt, dissolved the albumen, forming with it an alkali-albuminate.

The digestive action of the secretion on fat was finally tested. A few cubic centimetres of it were shaken up with a few drops of acid-free cod liver oil, but failed to emulsify them. Mixed, however, with slightly rancid or acid oil, it produced some degree of emulsion. This was evidently dependent on the alkalinity of the secretion due, probably, to the presence of carbonate of soda. A dilute solution of this salt formed a well-marked emulsion with the rancid, but none with the pure, oil. A fat-splitting ferment is generally believed to be present in the pancreatic juice, although Roberts<sup>1</sup> in his recent Lumleian lectures throws a doubt on its existence. And since the intestinal secretion failed to produce an immediate emulsification of fats, I endeavoured to ascertain, by digesting it for some time in an oven with oil, if it also contained this peculiar ferment. For this purpose some of the secretion was mixed with a few drops of pure acid-free cod liver oil. The mixture was slightly alkaline, and did not form an emulsion on shaking. After three hours the mixture was almost neutral, and formed a slight but distinct emulsion on shaking. A small quantity of the fatty acids had therefore been set free, which, becoming saponified by the alkali of the secretion, slightly emulsified the oil. If more of the fatty acids had been disengaged than there was free alkali to unite with, then the addition of a drop or two of the carbonate of soda should have converted them into soap and increased the degree of the emulsion. Carbonate of soda, although added to a portion of the mixture, did not, however, render the emulsion more distinct. Again placed in the digesting oven, and examined after other three hours, the mixture on being well agitated did not show a better emulsion than previously; but, although neutral litmus paper was not visibly reddened, the emulsion became much more

<sup>1</sup> Roberts, *Digestive Ferments*, 1880.

pronounced on the addition of a drop of a solution of carbonate of soda. The oil had, therefore, been partly decomposed by the intestinal fluid, but very slowly. The method I have employed for detecting the presence of free fatty acids by adding an alkaline carbonate, and observing the degree of emulsion produced on shaking, seems to me a much simpler and more efficient method than testing the acidity of the fluid as is commonly done. It is extremely difficult to obtain evidence of an acid reaction when there is only a trace of free fatty acids, and this difficulty has been the cause of many contradictory statements as to the fat-splitting power of even the pancreatic juice.

I have throughout this series of experiments regarded the action of the salt as purely local, and not appreciably involving any part of the nervous system outside of the intestinal wall. Professor H. C. Wood's experiments, of which I have already taken notice in the historical part of this paper,<sup>1</sup> appear to indicate that section of the vagi is capable of preventing purgative action by inhibiting secretion. He administered arsenic and croton oil to several cats with divided vagi without inducing purgation, but it is not stated in the short report of his experiments, which I have had the opportunity of perusing, whether he examined the intestines after the administration of the purge, and found them empty. The failure of these substances to purge might have been due as much to paralysed or even irregular peristaltic movements as to inhibited secretion. To decide whether the action of a saline purgative was similarly affected by division of the vagi the following experiment was made:—

*Experiment LIII.*—Cat, female, weighing 2.25 kilograms. Cut the vagi in the neck, carefully avoiding injury of the sympathetics, which in the cervical part of their course are closely united to the vagi. Immediately afterwards the abdomen was opened, and 10 c.c. of a 20 per cent. solution of sulphate of soda were injected into a collapsed and empty loop of the small intestine, which was previously ligatured. Killed after the lapse of *two hours*.

*AUTOPSY.*—The loop measured 63 cm. long, and contained 32 c.c. of a colourless, slightly opaque, viscid fluid, with the usual alkaline reaction.

Division of the vagi had not, therefore, in this experiment interfered with the excito-secretory power of a strong solution of

<sup>1</sup> *Supra*, p. 254.

a saline purgative. In how far it might have affected the peristalsis of the intestines, the experiment fails to show. But the deduction is important, that the salt can purge, in so far as it excites secretion, independently of the vagi, and that Wood's experiments do not prevent me from endeavouring, as I have done, to find in the intestine itself, and in the nature of the fluid injected and secreted, a sufficient explanation of the phenomena hitherto observed. Moreover, the results of Wood's experiments stand in contradiction to those obtained by John Reid,<sup>1</sup> who claimed to have observed that arsenic increased the intestinal secretion in animals with divided vagi.

With this experiment I conclude Series B., and in order to admit of a ready comparison of the results of the various experiments I subjoin them in a tabular form. Cats were employed for all the experiments, with the exception of XXXIV., in which a rabbit was used.

The main object of this series of experiments was to ascertain whether, in contradiction to the theory previously expressed, a purgative salt excites a flow of secretion from the intestine. A careful consideration of all the experiments leads but to one conclusion—that a purgative salt (sulphate of soda), whatever be its amount, or its strength of solution, invariably excites more or less secretion; and that, depending on the relative activity of absorption and secretion which processes proceed simultaneously in the intestine, the bulk of the injected saline solution increases, diminishes, or remains constant in amount.

The detailed results obtained are many of them striking, and I briefly recapitulate them:—(1) A 20 per cent. solution of the salt always excites a profuse secretion: (2) a 10 per cent. solution also increases in bulk, if injected in sufficient quantity; (3) but if in limited quantity (10 c.c. to 60 cm. of intestine) it does not increase; (4) whereas a 20 per cent. solution, containing the same quantity of the salt, and injected into the same length of intestine increases very largely in volume: (5) nevertheless, secretion is active in (3), as ascertained from the change in the characters of the fluid, but is balanced by absorption; (6) and while secretion may be more or less stimulated in (4), absorption is greatly impeded but not in abeyance, as Moreau would have us

<sup>1</sup> John Reid, *Physiolog. Anatom. and Pathological Researches*, 1848, p. 241.

believe; for the injected salt has diminished in quantity, and strychnia can still pass from the loop into the circulation: (7) after removal of the secreted fluid in (1) or (4), no more fluid accumulates within the loop; (8) but, although there is no

Number of Experiment.	Portion of Intestine Injected.	Length of Loop.	Quantity of Solution of Sulphate of Soda Injected.	Strength of Salt Solution.	Duration of Action.	Quantity of Fluid recovered from Loop.	Quantity of Na <sub>2</sub> SO <sub>4</sub> 10H <sub>2</sub> O recovered from Fluid.
		cm.	c.c.	per cent.		c.c.	grms.
XXXIII.	Ileum.	6	1	10	5 hours	8.45	...
XXXIV.	Ileum.	7.5	1	10	5	8.55	...
XXXV.	Ileum.	6	1	2½	3	0	...
	Colon.	2	1	2½	3	0	...
XXXVI.	Ileum.	3	1	5	5	0.8	...
	Colon.	3	1	5	5	3.5	...
	Ileum.	5.7	1	5	2	1.4	...
XXXVII.	Colon.	5.6	2	2½	2	1.0	...
	Colon.	3	1	5	2	1.2	...
	Ileum.	3	2	2½	2	1.2	...
XXXVIII.	Ileum.	6.1	1	10	2	3.2	...
	Ileum.	11.6	1	10	2	1.4	...
	Ileum.	8	1	10	5	0.8	...
XXXIX.	Ileum.	14	1	10	5	0.1	...
	Colon.	5.2	1	10	5	1.8	...
	Duodenum.	6.1	1	10	5	1.1	...
XL.	Jejunum.	6.5	1	10	5	1.05	...
	Ileum.	6.2	1	10	5	1.1	...
XLI. <sup>1</sup>	Ileum.	6	1	10	5	0.1	...
XLII. <sup>1</sup>	Ileum.	6.5	1	10	2	3.7	...
XLIII.	Ileum.	51	9	10	5	5.6	0.041
XLIV.	Ileum.	52	10	10	5	6	0.101
XLV.	Ileum.	54	9	10	2	9.9	0.232
XLVI.	Ileum.	54	20	10	5	34	...
XLVII.	Ileum.	52	4½	20	3	36	0.622
XLVIII.	Ileum.	60	4½	20	5	8	...
XLIX.	Ileum.	53	4½	20	5	54	0.824
L. <sup>2</sup>	Ileum.	52	10	20	54 mins.	36	1.382
LI. <sup>3</sup>	Ileum.	56	20	10	28	26	...
LII. <sup>4</sup>	Ileum.	...	...	...	...	...	...
LIII. <sup>5</sup>	Ileum.	63	10	20	2 hours	32	...

<sup>1</sup> Excessive number of ligatures.

<sup>2</sup> Death in 39 minutes after injection of strychnia into loop.

<sup>3</sup> Death in 13½ minutes after injection of strychnia into loop.

<sup>4</sup> *Vide* protocol.

<sup>5</sup> Vagi divided.

further accumulation of fluid, the relation of the secretive to the absorptive power of the loop is altered, the latter being weaker than the former, or the former relatively stronger than the latter, as compared with their ratio previous to the injection of the salt: (9) the local effect of a ligature applied to the



intestine is to excite secretion from the mucous membrane in its immediate vicinity, and therefore add to the bulk of the saline solution; (10) the reflex effect of a ligature, as exercised through the nervous system, is, on the contrary, to diminish the quantity of the secreted fluid, probably by stimulating and accelerating absorption: (11) division of the vagi does not affect the quantity or nature of the secretion: (12) all parts of the small intestine yield an equal amount of secretion when acted upon by the salt; (13) and the large intestine behaves in a similar manner towards the salt, excepting that, partly from the greater viscosity of its secretion, and partly from the nature of its mucous membrane, absorption proceeds with greater slowness than in the small intestine: (14) the secreted fluid does not contain more than a trace of albumen, and not more than is present in ordinary saliva, and cannot, therefore, be an inflammatory exudation, as Vulpian suggests: (15) on the other hand, it always contains much mucin, especially when obtained by the action of a 20 per cent. solution of the salt: (16) the fluid possesses, although in small degree, all the digestive properties attributed by most physiologists to the succus entericus—converting starch into maltose, inverting cane-sugar, splitting up fat, and dissolving, if not peptonising, albumen: (17) the mucous membrane is without exception pale, rather than congested, after the action of the salt—another fact opposing Vulpian's view: (18) and, finally, no increase in the vigour of the peristaltic movements of the injected loop of the intestine was ever visible.

Some of these results are of purely physiological interest, and help but little to solve the nature of the action of a dose of a saline purgative when administered in the usual manner. They, however, serve to determine the value of a method which has been much employed in recent years to ascertain the action of medicines and particularly of purgatives on the intestines, and, as yet, the only method by which it has been shown that a purgative salt excites secretion from the intestinal wall. They indicate how comparatively trivial alterations in the conditions of the method may greatly affect the issue of the experiment, and they will thus tend to render more accurate and trustworthy the work of succeeding investigators. Yet this method of Colin and Moreau is open to serious objection on account of

the necessarily great disturbance of the abdominal viscera occasioned by their exposure; and while, as I have already stated, it clearly leads to the conclusion that the purgative salt excites secretion within the intestine, proof must still be furnished of the truth of this occurrence by such methods as will be free from the disturbance of an abdominal operation. The next two series of experiments supply this want; and until these have been presented no attempt will be made to explain and reconcile the opposing results of the experiments of this and the preceding series.

### SERIES OF EXPERIMENTS, C.

The effect of saline purgation on the concentration of the blood.

The first series of experiments tended to show that the salt when administered without water, or with insufficient water, was not capable of purging; and the conclusion naturally was that the salt did not excite secretion, or draw fluid from the blood. But, as in order to free the alimentary canal from fluids the animal received no water with its food for one or more days previous to the administration of the salt, it was possible that in addition to the canal being freed from its water, the blood had become considerably concentrated from the deprivation of its water-supply, while the kidneys continued to secrete almost as abundantly as usual. The concentration of the blood thus effected might have resisted the abstraction of fluid by the salt, and have constituted the principal if not the sole cause of the absence of purgation when the salt was administered with little or no water. I have already suggested this explanation, and I shall offer the proof of its probability in the course of the following experiments.

One other fact of primary importance is brought out by these experiments. Whatever action a diluted solution of the salt may have in exciting intestinal secretion, the strongest evidence will be furnished of a concentrated solution abstracting a large amount of fluid from the blood.

In order to ascertain the degree of concentration of the blood, the number of corpuscles in a given quantity of it was from time to time counted according to the method introduced by Vierordt, and afterwards improved by Malassez and Hayem. I reckoned an

increase in the number of the corpuscles as equivalent to a loss of serum, and a diminution as indicating dilution of the blood.

The relation of the number of the blood-corpuscles to the quantity of the serum is greatly altered, as is well known, in many diseases, notably in anæmia. These alterations are believed to be almost entirely due to the diminution or increase of the total number of the blood-corpuscles in the body, the serum undergoing no alteration in quantity. There are, however, physiological variations of daily occurrence, which are to be attributed to no change in the number of the corpuscles, but to the diminution or increase of the liquor sanguinis, and are mostly the effect of diet and digestion. Thus, after a meal, Sörensen, Dupérié, Buntzen, and others have shown that the blood becomes more concentrated from a large part of its water passing out in the alimentary secretions—the gastric, pancreatic, and intestinal juices, and the bile. The concentration will be less, or not at all, discernible, according as little or much fluid is taken with the food; for that being absorbed will take the place of the fluid which has been poured out in the secretions. When the food after digestion begins to be absorbed and pass into the circulation, the concentration of the blood is relieved.

If the food can by its withdrawal of water from the blood make this ascertainable through the relative increase of the corpuscles in the blood, it is to be expected that a purgative salt, if it excites secretion, should have its action similarly registered by a concentration of the blood. But, as when much fluid is consumed with the food, no concentration of the blood occurs, so it may be anticipated that only a strong solution of the purgative salt will thus affect the blood, a weak solution permitting of an absorption which equals in amount the secretion.

Gower's modification of Hayem's instrument was that used for measuring and diluting the blood and counting the corpuscles. Two separate enumerations of the corpuscles were always made, and in the first two experiments one of these enumerations was made by Dr. Logan, who kindly assisted me, and the other by myself. Ten of the microscopic square areas were always included in each counting.

*Experiment LIV.*—J. W., male, æt. 33. Partook of a light meal of bread and milk at 11 A.M. At 3.5 P.M. was placed in bed, and at

3.25 P.M. a drop of blood was withdrawn in the usual manner from the thenar eminence of the right hand. Care was taken that the skin of the hand was perfectly clean at the point punctured and free from moisture. If the blood did not come freely, it was never pressed out, as by pressure the interstitial fluid of the tissues was apt to be mixed with the blood; and if without pressure not more than a single drop oozed out it was rejected, and another and deeper puncture made; for, again, was there a chance of its contamination with the tissue-fluid from its slowly appearing. At 3.38 P.M. a second enumeration of the blood-corpuscles was made for the purpose of controlling the first, and two minutes afterwards a purgative dose of sulphate of soda in a concentrated form was administered. The blood was examined several times afterwards. All the corpuscles, both red and white, were counted, and the numbers obtained throughout the experiment were as follows:—

3.25 P.M.—4,850,000 corpuscles in each c.mm. of blood.

3.38 P.M.—5,020,000

3.40 P.M.—Administered 21.3 grammes ( $\frac{3}{4}$  oz.) of sulphate of soda dissolved in 85 c.c. (3 oz.) of water, or about a 20 per cent. solution.

4.15 P.M.—6,540,000 corpuscles in each c.mm. of blood.

4.55 P.M.—6,790,000

5.20 P.M.—6,610,000

6 P.M.—5,710,000

6.45 P.M.—5,740,000

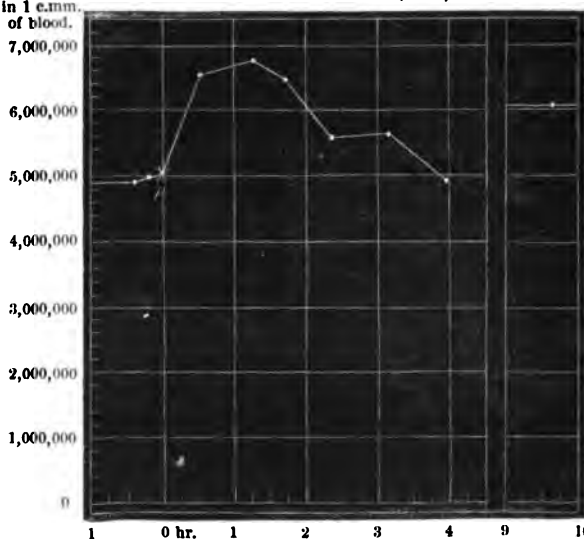
7.40 P.M.—4,930,000

7.45 P.M.—Dinner.

1 A.M.—6,020,000

No. of  
corpuscles  
in 1 c.mm.  
of blood.

EXPERIMENT LIV. (Man).



Showing the effect of the administration of a 20 per cent. solution of sulphate of soda on the concentration of the blood.

The first evacuation of his bowels occurred at 6 A.M. on the following morning, and three hours afterwards there was a second fluid defecation.

These numbers show that very shortly after the administration of the salt the blood has become highly concentrated, the result without doubt of the rapid loss of fluid which it has suffered during the action of the salt in the alimentary canal. The degree of concentration is remarkable, and reveals to what extent the quantity of the liquid constituents of the blood may be reduced without being manifested by any corresponding outwardly visible symptoms. If the enumerations were made correctly, and every care was taken to ensure that they were, and if the numbers of the corpuscles truly represent to what extent the blood has been deprived of its fluid, it can be readily calculated that the total quantity of the blood had become reduced at 4.55 P.M. from 1 to 0.73; and supposing the man weighed 140 lbs., and that 10 lbs., or the  $\frac{1}{14}$ th part of this weight, were blood, then 2.7 lbs. of fluid must have passed into the alimentary canal.

But what is almost as remarkable is the rapidity with which the blood has returned to its normal state of dilution. This it accomplishes within four hours after the administration of the salt, and without any fluid having been taken in the interval. We cannot believe that the fluid at first poured out by the blood into the alimentary canal was afterwards absorbed, else how did purgation occur next morning. The dilution of the blood is only to be explained by the concentrated blood abstracting water from the tissues, in virtue both of its concentration and of its endosmotic power having become increased by the presence of the absorbed sulphate of soda.

Another noteworthy point is the concentration of the blood an hour after midnight. This secondary concentration of the blood I have observed in all my experiments, and I believe it exists during the greater part of the day following the administration of the salt, and long after the salt has ceased to excite alimentary secretion, and is doubtless to be attributed to the diuresis created by the absorbed salt in the process of its elimination by the kidneys. The diuretic effect ought certainly to begin so soon as absorption of the salt occurs, that is, almost

immediately after administration; but it is probable that the absorption of the salt proceeds slowly, and its full diuretic action is not observable until after some time. Moreover, after the drain upon the blood by the salt within the intestines has been arrested, the tissue-fluids will for a while be able to balance the diuresis; but, when the tissues begin to yield their fluid more slowly from their comparative exhaustion, the diuresis, proceeding with the same rapidity as previously, will lead to a concentration of the blood. The quantity of urine evacuated before and after purgation supports this view.

Day before the experiment,	. . .	1596 c.c.
Day of the experiment,	. . .	1316 "
First day after the experiment,	. . .	2240 "
Second day after the experiment,	. . .	1120 "

The next experiment was made a few days afterwards on the same individual. On this occasion a dilute solution of the same quantity of the salt was administered.

*Experiment LV.*—The blood was taken from the thenar eminence of the right hand as in the previous experiment. At 11 A.M. he was supplied with a light diet of bread and milk, and at 3.20 P.M. he was placed in bed and the experiment commenced. The following is the record of the various enumerations of the blood corpuscles before and after the administration of the purge.

3.40 P.M.—5,250,000 corpuscles in each c.mm. of blood.

3.55 P.M.—Administered 21·3 grammes ( $\frac{3}{4}$  oz.) of sulphate of soda dissolved in 227 c.c. (8 oz.) of water, followed immediately by other 227 c.c. (8 oz.) of water, altogether about a 5 per cent. solution.

4.20 P.M.—5,140,000 corpuscles in each c.mm. of blood.

5.10 P.M.—5,310,000 " " "

5.32 P.M.—5,300,000 " " "

5.40 P.M.—Free watery purgation.

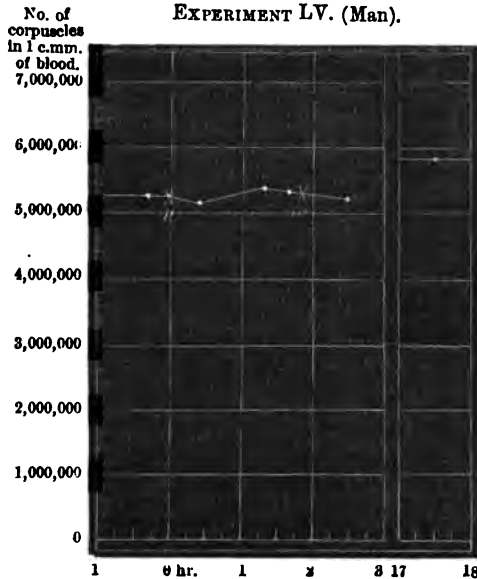
6.22 P.M.—5,235,000 corpuscles in each c.mm. of blood.

Next morning and before breakfast:—

9.30 A.M.—5,830,000 corpuscles in each c.mm. of blood.

The dilute solution of the sulphate of soda, unlike the concentrated solution, caused no immediate concentration of the blood; but here, also, as with the other, there followed a remote hæmatic concentration, which was naturally to be expected if the explanation I have already given of this concentration be correct, an

explanation supported once more by the quantity of urine excreted.



Showing the effect of the administration of a 5 per cent. solution of sulphate of soda on the concentration of the blood. Purgative administered at *p*; purgation occurred at *m*.

Day before the experiment, . . . 1092 c.c.

Day of the experiment, . . . 528 "

(The urine evacuated at 5.40 P.M., when purgation occurred, is not included.)

Day after the experiment, . . . 1736 "

A dilute solution of the salt passes, therefore, through the canal without, in so far as its purgative effect is concerned, producing any change in the volume of the blood. The remote concentration of the blood, due, as I believe, to diuresis, is, however, almost as evident as in the previous experiment.

We are warranted in concluding from these two experiments, contrary to the view expressed in the first series of experiments, that, at any rate, a concentrated solution of a saline purgative excites secretion within the alimentary canal sufficient to produce a marked diminution of the volume of the blood. Thus, by a method less open to objection than that employed in the

preceding series of experiment, we have arrived for the concentrated salt at the same result.

The two succeeding experiments are merely a repetition on the dog of those I had made on man, as I deemed it desirable to have confirmation of the interesting results obtained.

*Experiment LVI.*—Terrier bitch, weighing 6.12 kilograms. No food on day of experiment. The outer surface of the calf of the right fore-leg was cleanly shaved, and from this, by puncturing with a sharp needle, sufficient blood was obtained for examination.

3.20 P.M.—5,530,000 corpuscles in each c.mm. of blood.

3.45 P.M.—Administered 12 grammes of sulphate of soda made into large pills with bread and a few drops of mucilage. This quantity dissolved in water had been found sufficient a week previously to purge the animal.

4.15 P.M.—6,730,000 corpuscles in each c.mm. of blood.

4.45 P.M.—6,500,000 " " "

5.45 P.M.—6,380,000 " " "

7.20 P.M.—5,860,000 " " "

8 P.M.—5,680,000 " " "

8.10 P.M.—Fed as usual.

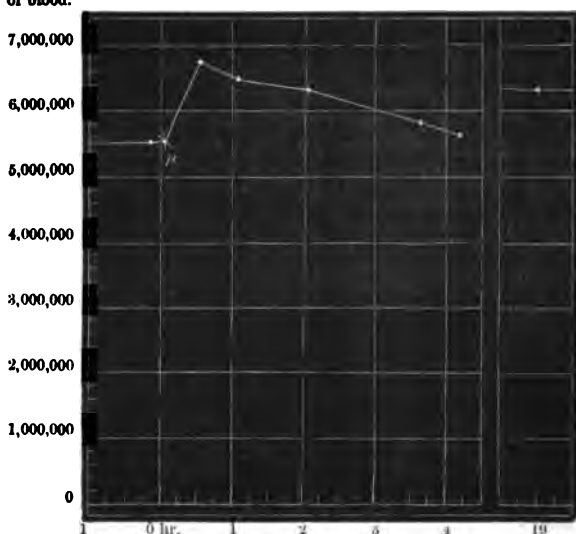
Next day, and before receiving food :—

10.45 A.M.—6,320,000 corpuscles in each c.mm. of blood.

5 P.M.—Fluid evacuation of the bowels.

No. of  
corpuscles  
in 1 c.mm.  
of blood.

EXPERIMENT LVI. (Dog).



Showing the effect of the administration of a 20 per cent. solution of sulphate of soda on the concentration of the blood. Purgative administered at *p*.



The result of this experiment is practically the same as that of Experiment LIV., except that the concentration of the blood is quite so excessive.

*Experiment LVII.*—Same dog as in preceding experiment, but several days afterwards. No food on day of experiment. Blood taken from the same part of the body as before.

11.30 A.M.—5,490,000 corpuscles in each c.mm. of blood.

11.35 A.M.—Administered 12 grammes of sulphate of soda dissolved in water so as to form a 5 per cent. solution.

12 noon.—5,300,000 corpuscles in each c.mm. of blood.

12.23 P.M.—5,310,000       "       "       "

1.10 P.M.—5,650,000       "       "       "

2 P.M.—5,350,000       "       "       "

3 P.M.—5,430,000       "       "       "

4.30 P.M.—5,720,000       "       "       "

6 P.M.—5,690,000       "       "       "

6.20 P.M.—Fed as usual.

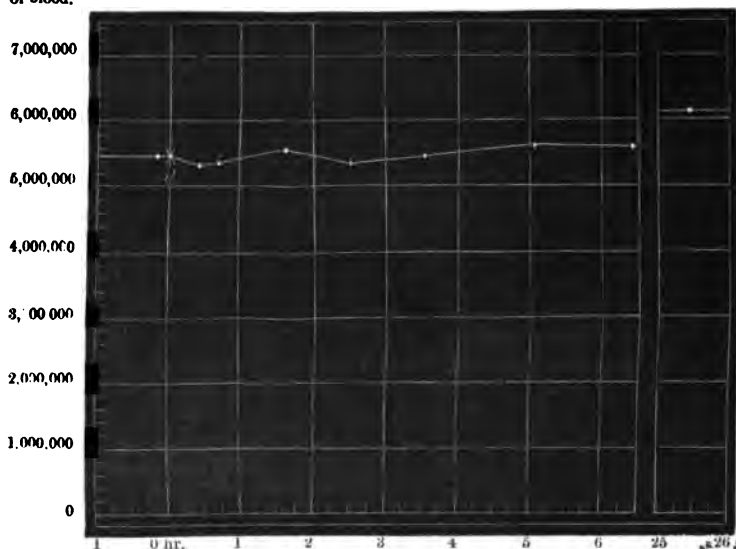
Next morning, and before feeding :—

10.15 A.M.—6,110,000 corpuscles in each c.mm. of blood.

Purgation took place during the night.

No. of  
corpuscles  
in 1 c.mm.  
of blood.

EXPERIMENT LVII. (Dog).



Showing the effect of the administration of a 5 per cent. solution of sulphate of soda on the concentration of the blood. Purgative administered at *p*.

Here, again, as in the corresponding experiment on man, a

dilute solution of the purgative does not produce an immediate concentration of the blood, but does so after many hours.

From the next experiment, which concludes the present series, we learn that if a dog be allowed no water for two days the blood will become concentrated; and if, in this condition, a concentrated solution of a purgative salt be administered, little or no further concentration of the blood will occur, showing that in such a condition the blood is no longer capable of parting with its water to the secretion excited by the salt, or parts with it in a quantity insufficient to produce purgation. An explanation is thus offered of the results obtained from my first series of experiments.

*Experiment LVIII.*—Same dog as in the two preceding experiments. Fed for two days on stale wheaten bread; no water. No food on day of experiment. Blood taken from the same part of the body as previously.

Before the commencement of the water-restricted diet, the corpuscles in one c.mm. of blood numbered 5,510,000. On the third day of the special diet, they were as follows:—

5.10 P.M.—6,910,000 corpuscles in each c.mm. of blood.

5.20 P.M.—6,800,000

5.35 P.M.—Administered 12 grammes of sulphate of soda made into pills with bread and a drop or two of syrup.

6.10 P.M.—7,130,000 corpuscles in each c.mm. of blood.

6.30 P.M.—7,210,000

6.55 P.M.—7,120,000

7.3 P.M.—Vomited between 20 and 30 c.c. of "excessively tough, glairy, colourless fluid, a small portion being slightly tinged with blood. It was so viscid that, after standing for half an hour, it could hardly be poured out of the vessel in which it had been collected. It was mixed with a large quantity of undissolved crystals of the sulphate. The reaction was acid, but not so strongly acid as the gastric juice usually is."

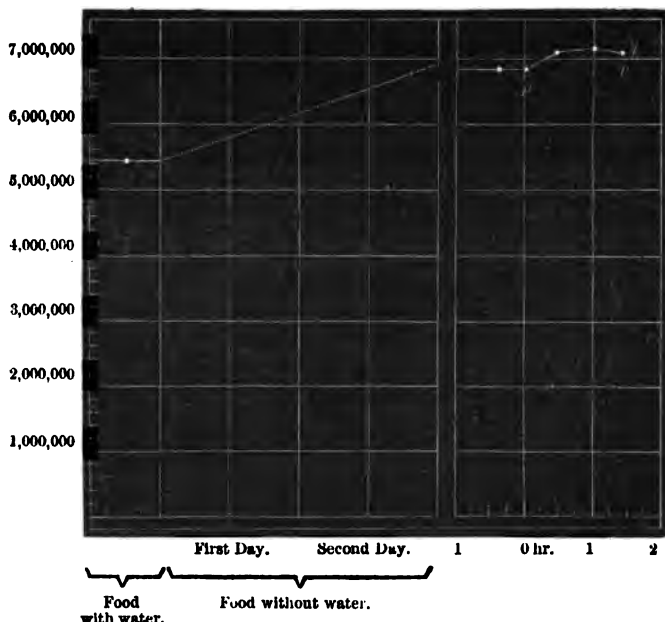
Purgation never occurred, although water was given to the dog immediately after it vomited.

The blood, notwithstanding its previous concentration from a restricted water-supply, loses a small portion of its fluids, probably as much as the salt has required for its solution and absorption. The pill form in which the salt was given did not, I think, delay its action, as the pills were made only a few

minutes before administration, and were very friable, breaking down with the gentlest pressure.

No. of  
corpuscles  
in 1 c. mm.  
of blood.

## EXPERIMENT LVIII. (Dog).



Showing the effect on the concentration of the blood,—of (1) abstinence from water, and (2) the administration during this condition of a purgative dose of sulphate of soda in a concentrated form. Purgative administered at *p*; vomiting occurred at *v*.

The result of this experiment explains how, in the first series of experiments, purgation failed to be produced by the administration of a concentrated solution of a saline purgative to an animal which had received no water for two days previously. By the restriction of water in the diet, not only was the alimentary canal emptied of its fluids, but the blood was also greatly concentrated. And, notwithstanding Experiment XXIX., where, indeed, a little purgation did occur, I am strongly inclined to think that the concentration of the blood was a much more powerful factor in the prevention of purgation than the absence of fluid in the alimentary canal. A single night's fasting is sufficient to deprive the canal of fluid, as I have ascertained

whilst making the experiments of the succeeding series; and after so short an abstention from water, I have never found that cats were to any extent less powerfully purged by a concentrated solution of the salt than by a dilute solution, although it has been proved in Series A. that the former failed to act when water was withheld from the animal for two days or more. This implies, indeed, that the method employed in Series A is not satisfactory, but it still leaves us the very interesting fact, that when the blood becomes concentrated it no longer yields secretion to the stimulus of a saline purgative applied to the mucous membrane of the alimentary canal. It would appear to be otherwise when the secretion is the result of inflammatory irritation, if the result of the experiment with croton oil (Experiment XXX.) can be proved to be constant.

In justice to M. Brouardel, I ought to mention that he has preceded me in the study of the action of cathartics on the concentration of the blood, although I was not aware of the existence of his paper (*L'Union Médicale*, No. 110, t. xxii.) when I made the above experiments. The conditions of his experiments are, however, so lax, that the absence of any account of them in physiological and pharmacological text-books is readily understood. His investigation was conducted on patients in the hospital of St. Antoine. The purgatives employed were castor oil, croton oil, jalap, and in one instance a saline solution (*Eau de Sedlitz*), and the degree of concentration of the purgative was not attended to. The blood was examined some time in the afternoon, and apparently without regard to the effect of ordinary meals on its concentration. The purgative was shortly afterwards administered, and the blood was not again examined until some time on the following day, so that the concentration he observed was that which I have pointed out as secondary and not immediately dependent on the cathartic action of the salt. Moreover, in nearly every case, eight altogether, his patients were suffering from diseases which might of themselves have seriously affected the condition of the blood when under the influence of purgatives—heart disease and anasarca in two cases, prolonged constipation (eight days, &c.), in four others. He adds, in opposition to Ch. Robin,<sup>1</sup> that purgatives diminish instead of notably augmenting the number of the white corpuscles in the blood.

<sup>1</sup> Ch. Robin, *Leçons sur les humeurs normales et morbides*, Paris, 1867, p. 52.

(To be continued.)

THE BOSTON  
SOCIETY FOR  
MEDICAL  
OBSERVATION

ABNORMAL ORIGIN AND DISTRIBUTION OF THE  
UPPER SEVEN RIGHT INTERCOSTAL ARTERIES,  
WITH REMARKS. By W. J. WALSHAM, F.R.C.S.,  
*Assistant Surgeon to, and late Demonstrator of Anatomy at  
St. Bartholomew's Hospital; Examiner in Anatomy in the  
University of Aberdeen.*

THE right superior intercostal artery arose from the first portion of the subclavian opposite the vertebral, and after giving off the deep cervical branch in the normal manner, crossed in front of the neck of the first rib external to the inferior cervical ganglion of the sympathetic into the first intercostal space as usual. Thence turning backwards between the necks of the first and second ribs it passed downwards between the neck of the second rib and the transverse process of the second dorsal vertebra, piercing the middle costo-transverse ligament. Continuing its course across the second intercostal space, it passed in like manner between the neck of the third rib and the transverse process of the third dorsal vertebra; and then turning forwards between the necks of the third and fourth ribs, anastomosed in the third intercostal space with a large branch given off from the thoracic aorta opposite the seventh dorsal vertebra. At the spot where these vessels anastomosed the artery to the third intercostal space took its origin. The branches from the superior intercostal in the above course were as follows:—

1st, As it crossed the first intercostal space it gave off (a) a branch from its outer side, which was distributed as the first intercostal in the normal manner; (b) a branch from its inner side, which ran through the intervertebral foramen along the course of the spinal nerve into the spinal canal; (c) a branch from behind which proceeded through the quadrate foramen in the usual manner to the muscles of the back; (d) a small branch from the front at the spot where it dipped behind the neck of the second rib; this branch anastomosed with a similar branch given off from the superior intercostal artery in the second intercostal space, immediately below the second rib, and formed with it an anastomoic loop around the front of the head of the rib, from

which loop very small arterioles were distributed to the bodies of the vertebræ, the intervertebral substance, and the anterior common ligament.

2nd, As the superior intercostal artery crossed the second intercostal space it gave off branches similar to those given off by it in the first space ; and an anastomotic loop was formed over the front of the head of the third rib, in all respects like



Drawn from nature by W. J. Walsham.

that formed over the head of the second rib, and from this loop branches were distributed in the same manner as those from the first loop. One of these branches, slightly larger than the rest, anastomosed with a small twig from the back of the thoracic aorta. This twig occupied the usual position of the first aortic intercostal, and also supplied branches to the vertebræ and to the structures in the posterior mediastinum.

The large branch with which the superior intercostal anastomosed in the third space was derived from the aorta opposite the seventh dorsal vertebra. It first ran outwards and slightly upwards, and then turning more directly upwards, ran across the bodies of the seventh sixth, and fifth vertebræ, and the heads of the fifth and fourth ribs to the third space to join the superior intercostal. In this course it supplied branches which were distributed

normally to the seventh, sixth, fifth and fourth intercostal spaces. From its inner side three very delicate arterioles anastomosed with similar fine vessels derived from the aorta.

The superior intercostal artery has been observed passing behind the necks of the second and third ribs ; but such a course must be rare, as this is the first instance that has

occurred during my eight years' connection with the dissecting room of St. Bartholomew's Hospital. Two or more intercostal vessels have also been observed arising from a single trunk. But I am not aware that the combination of abnormalities of the intercostal arteries above described has been previously recorded.

The passing of the superior intercostal artery between the neck of the rib and the transverse process of the vertebra is of considerable interest. The artery would here appear to be homologous to the vertebral artery in the neck, as both lie between homologous parts,—the anterior part of the transverse process of the cervical vertebra being the homologue of the neck and head of the dorsal rib, and the posterior part of the cervical transverse process that of the transverse process of the dorsal vertebra. The abnormal artery, moreover, like the vertebral, lay in front of the spinal nerves. A comparison may also be made between the distribution of the branches of the abnormal intercostal and those of the vertebral. Both send spinal branches through the intervertebral foramina to supply the spinal cord and membranes. Both supply the muscles of the back in their respective regions. Both give off external lateral branches—the intercostal to the intercostal muscles—the vertebral to the scalene (which may be regarded as the homologues of the intercostals in the neck). Both give off small twigs to form an anastomotic loop,—in front of the neck of the rib in the one situation, and in front of the homologous part,—the anterior transverse process—in the other situation.

Normally in a well injected subject<sup>1</sup> a small twig from each intercostal can be traced running between the neck of the rib and the transverse process of the corresponding vertebra where it anastomoses with a similar twig given off from the intercostal artery next below.<sup>2</sup> In the first and second spaces similar anastomosing twigs are given off from the superior intercostal.

The abnormality as regards the superior intercostal, then

<sup>1</sup> Better demonstrated in a foetus.

<sup>2</sup> No account of these anastomoses, as far as I know, is given in the works on anatomy, but my friend and former pupil, Mr. S. P. Pruett, the present Senior Assistant Demonstrator of Anatomy at the Newcastle School of Medicine, who has investigated the point for me in a number of cases, has invariably found them to be present.

would appear to depend upon an enlargement of the normal anastomotic loop which runs between the transverse process and the neck of the rib, and upon the shrinking of that portion of the vessel which ordinarily courses in front of the neck of the rib. The delicate loop in front of the neck of the rib would then represent the shrunken portion of the normal artery. The large branch given off from the aorta and anastomosing with the superior intercostal in the third space would in the same way appear to be due to an enlargement of the normal anastomosing branches which exist between the several intercostal vessels; whilst the delicate arterioles joining the abnormal vessel to the aorta may be regarded as the shrunken remains of the normal intercostals.

The somewhat similar abnormalities of the superior intercostal and vertebral arteries which have been observed may be explained in like manner. Thus, in that instance where the superior intercostal artery is given off from the vertebral, and passes through the foramen in the transverse process of the seventh cervical vertebra, and between the neck of the first rib and the transverse process of the first dorsal vertebra into the first intercostal space, to anastomose with the first aortic intercostal, the abnormality would seem to depend upon the enlargement of the normal anastomotic branch which passes between the vertebral and the deep cervical branch of the first intercostal. Again, in the instance in which the vertebral enters a higher or a lower foramen in the transverse process than usual, a small branch in the position of the vertebral—clearly its shrunken remains—can always be found if carefully sought for; whilst in the normal condition small anastomotic loops are always present in front of the transverse processes.

In the lumbar and sacral regions<sup>1</sup> an anastomosis between the lumbar arteries and between the lateral and middle sacral, resembling that in the dorsal and cervical, likewise normally exists. Thus along the whole length of the spine a double and

<sup>1</sup> In the dorsal region there is not only a small anastomotic loop passing between the neck of the rib and the transverse process of the vertebra, but also one passing behind the transverse process. The arrangement in the lumbar and sacral regions I have not yet clearly determined. But of these anastomoses, which for some time I have been working at in conjunction with Mr. Pruett, I hope at some future date to publish a full account.



perhaps treble series of anastomosing vessels may be said to occur. And I believe most of the abnormalities that have been observed in the vertebral, intercostal, and probably in the lumbar and sacral arteries, may be explained on the supposition of the enlargement, or complete or partial suppression of one or more of these anastomosing vessels, which I venture to call the lateral spinal.

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DESCRIPTION OF FIGURE.

- A. Aorta.
- B. Abnormal superior intercostal artery.
- C. Abnormal branch from aorta.

ON THE DIGESTION OF BLOOD BY THE COMMON LEECH, AND ON THE FORMATION OF HÆMOGLOBIN CRYSTALS. By WILLIAM STIRLING, M.D., Sc.D., *Professor of the Institutes of Medicine*, and PHILIP S. BRITO, *Student of Medicine in the University of Aberdeen.* (PLATE XII.).

PART I.—*Digestion of the Blood of Frog, Newt, Man.*

THE present research was undertaken with a view to ascertain the changes which blood and blood corpuscles undergo within the alimentary canal of the common leech (*Hirudo officinalis*). In carrying out this object we were forced to undertake certain experiments to ascertain what effects are produced upon blood corpuscles of various animals by the addition of various reagents.

It is extremely difficult to obtain leeches whose alimentary canal does not contain blood, or the products of the digestion of the blood; and therefore it is necessary to keep the leeches with which one experiments for a considerable time—several months—before allowing them to suck. In carrying out these experiments, we have used animals whose blood corpuscles are easily recognisable, such as the frog and newt; but we have also made some observations with human blood.

BLOOD OF THE FROG.

On November 20, 1880, we placed two leeches in water in a bottle along with a pithed frog. The leeches very soon fastened upon their victim; and, notwithstanding the vigorous reflex movements of the frog, they retained their hold. They fixed their suckers usually, but not invariably, to the skin of the armpit. They usually adhered firmly for a day or more; and when they were removed—from comparing their weight before and after sucking—we found that each leech had increased about a gramme in weight. The leeches were carefully removed and kept in water, and from time to time a drop of blood was expressed from the mouth and subjected to careful

microscopic examination. At other times we resorted to chemical and electrical stimulation to cause our leeches to disgorge their blood.

Microscopic examination of the blood on the *second* and *fourth* days showed that the blood corpuscles had not undergone any marked or very obvious change. On the *fourteenth* day we obtained two samples of blood; one was expressed forcibly, while the other was obtained by applying an 8 per cent. solution of common salt to the leech, care being taken that the reagent was not allowed to mingle with the blood. The blood was of a very deep dark-red colour, of jelly-like consistence, and was very stringy, probably from the admixture of mucus, which we found abundantly in all the specimens of blood which we examined. The dark, almost deep port wine colour of the blood was due to the reduction of the oxyhæmoglobin to the condition of reduced hæmoglobin, as shown by micro-spectroscopic investigation.

Innumerable *coloured* blood corpuscles filled the field of the microscope, and many of them were unchanged in their shape, while others had lost their oval outline, and become globular or somewhat irregular in their shape. The nuclei were apparently smaller and more globular. In some of the corpuscles the peri-nuclear part had become paler, so that the outline of these corpuscles was less obvious, while the fluid in which the corpuscles floated had a yellowish tinge from the admixture of hæmoglobin. The *colourless* corpuscles stood out brightly and highly refractive as usual, and they did not seem to have undergone any change except that their elasticity seemed to be diminished somewhat.

Numerous crystals were also to be seen. These *crystals* were colourless, highly refractive, acicular, and pointed at one extremity like the point of a pen. They were variable in size and length, and were scattered irregularly over the field of the microscope.

A *month* afterwards the coloured and colourless corpuscles were found in much the same condition as described above; but the crystals were more numerous, still uncoloured, and arranged, some singly, while others were grouped into sheaves, several crystals radiating from a single point; while, again, the larger

crystals had a number of smaller ones springing from the two sides at one end, like a quill for writing. The various forms assumed by these crystals are shown in Plate XII. fig. 3.

In almost all the samples of blood we examined we found striped muscular fibres still undigested. These fibres had been cut out of the frog by the leech with its triradiate teeth.

Similar experiments were made with other leeches, and the digested blood was withdrawn from them from time to time. Thus a leech was allowed to suck a frog on November 16, 1881 and the blood was examined on February 22, 1882; *i.e.*, ninety-eight days afterwards.

The blood expressed or vomited was thick and very dark red. On examination numerous globular nuclei, somewhat less in size than the nuclei of the coloured corpuscles, were seen. These were very probably the nuclei of the coloured corpuscles, which by this time had been so acted upon by the digestive juices as to have all their hæmoglobin, and perhaps some of their other constituents, extracted from them. In some cases the nuclei had around them the vestiges of the stroma of the corpuscles; for in a similar preparation, on adding water, a faint outline could be seen considerably outside and beyond the nuclei. This seemed to be due to the swelling up of the residue of the corpuscular stroma under the influence of the water.

Nuclei have a great power of resisting the action of various reagents and even digestive fluids, as one of us,<sup>1</sup> Kühne, and others have shown, in connection with the nuclei of the cells of connective tissue, nerves, &c. A similar condition seems to obtain in connection with nuclei of blood, in resisting for months the action of the digestive fluids of the leech.

Some *colourless* corpuscles were still to be seen, and they seem to be even more resistant to the digestive action than the coloured ones. Numerous acicular colourless crystals were obvious, and their characters were exactly the same as when they were examined at an earlier date.

The serum or fluid in which the corpuscles were suspended was yellowish in colour, which was due to the hæmoglobin which had passed out of the corpuscles, thus colouring the fluid.

The crystals are readily tinged with a watery solution of

<sup>1</sup> W. Stirling, *Journal of Anat. and Phys.* vol. x.

magenta, fuchsin, malachite green, iodine green, and methyl aniline; but we do not find that picro-carminine diffuses into them so as to stain them. They do not dissolve on the addition of water, but speedily disappear when dilute acetic acid is added.

The leech in sucking had detached a few muscular fibres from the muscles of its prey; and even after ninety-eight days these microscopic morsels of muscular fibres were still found striated and apparently unaltered. The hæmaglobin in solution had not even diffused into them.

The blood when vomited was not only very dark in colour, but it was semi-fluid. We kept some of it for twenty days, but it showed no tendency to coagulate or even to putrefy. About the twenty-fifth day a slight odour of putrefaction was observable, but even then the crystals remained unaffected. A sufficiently thick stratum invariably gave the spectrum of reduced hæmoglobin.

The process of digestion of blood in the leech, therefore, is an extremely slow one, as many of the corpuscles of the frog were unaltered after a month's sojourn in the intestinal canal of the leech; while even after three months some coloured corpuscles and the nuclei of other coloured corpuscles, and many of the colourless ones, were found still undissolved. Our observations on leeches do not enable us as yet to speak authoritatively as to what ultimately becomes of the nuclei and the dissolved out hæmoglobin. We have, however, examined the excreta of the leech, which is relatively small in amount, but we have never seen any trace of a coloured blood corpuscle, or even a nucleus of one, whilst invariably small yellowish semi-crystalline masses of pigment are to be found; and when these masses are larger they are brownish or even black in tint, but what their exact nature is we have not yet determined. They seem to be derived from the hæmoglobin, but as yet we have not succeeded in tracing all the intermediate stages which lead up to these crystals. It is clear, however, that after a period of more than three months the hæmoglobin is altered, and the nuclei disappear. The leech casts its cuticular epithelium in the form of a slough very often, and one must be careful not to confound it with the true intestinal excretion.

Our researches were specially directed to the investigation of

the crystals which made their appearance in the blood of the frog; and in order to ascertain how they arose, we mixed five or six drops of frog's blood, freshly drawn from the heart, with one or two drops of distilled water, and sealed up the preparation very carefully with gold size. Exposure to the air favours the formation of the crystals, and they form first around and in the neighbourhood of any coagula present. After several days, crystals similar to those found in blood obtained from the intestinal canal of the leech were found in the preparation; at first they were small and not very numerous, but after the slide had been put aside for two or more weeks the crystals became much larger, more numerous, and were arranged with smaller ones on one end, and they were exactly the same in every respect as those formed in the blood which had been swallowed by the leech. These crystals are always colourless, and can readily be produced by the means we have described. On referring to the literature of this subject, more especially to Preyer's work,<sup>1</sup> we find that Teichmann<sup>2</sup> obtained colourless crystals from frog's blood by mixing defibrinated blood with a large quantity of water, and allowing a drop to evaporate at a low temperature. As the crystals he obtained were colourless, it is doubtful if they consisted of hæmoglobin. The ease with which these crystals are obtained, the fact that they are formed by adding water to blood, and also in the intestinal canal of the leech, are points in which they agree with hæmoglobin crystals derived from the blood of other animals. The absence of colour, however, makes one hesitate to pronounce them hæmoglobin. In the case of one leech, however, on expressing some blood on the fourth day, after it had fed upon frog's blood, we obtained blood which, when sealed up and allowed to stand, developed beautiful crystals of exactly the same shape as those described above, but they were coloured. In this case the crystals were very much larger, extending quite across the field of the microscope when examined with 70 diameters. The sole difference was in the colour. We are therefore inclined to regard our colourless crystals as something very closely related to hæmoglobin, if they are not identical with it. At present we are unable to state

<sup>1</sup> *Die Blutcrystalle*, Jena, 1871.

<sup>2</sup> *Zeitschr. f. rat. Med.* 1853.

what is the ultimate fate of these colourless crystals which form within the alimentary canal of the leech from frog's blood. The crystals, both coloured mammalian and colourless ranine, retain their form and characters for months. In one specimen of frog's blood to which water was added we found crystals of a different shape—oblong prisms—but colourless. They grew to a considerable size, and can easily be seen with 70 diameters, when they form a marked colourless object in a field everywhere tinged with hæmoglobin, except where these prismatic or acicular crystals are formed.

#### BLOOD OF NEWT.

Leeches were placed in a vessel with water along with pithed newts. The leeches fastened greedily, usually on the soft ventral surface of their prey, and remained attached for a day. On removing them we usually found that the leeches had increased about a gramme in weight, *i.e.*, they had sucked about one gramme of blood. On the *ninth* day a drop of blood was expressed, and it was found crowded with coloured corpuscles, some of them slightly altered in shape, although their colour was in some cases of a dusky red, like the colour of reduced hæmoglobin, while the colourless corpuscles seemed quite unaffected. Here also we found striped muscular fibres of the newts. No crystals of any kind were observed; and even after the blood had been sealed up in the usual way for five days, no crystals were obtained. The blood expressed, as in the case of the frog, was dark and dusky in hue, and gave a decidedly marked spectrum of reduced hæmoglobin.

#### HUMAN BLOOD.

On Dec. 27, 1880, a leech was applied to the forearm for two hours.

Dec. 31, or *four* days thereafter, a drop of blood was expressed and examined, when the coloured corpuscles were found slightly smaller than natural, slightly paler in colour, but otherwise unchanged, while the colourless corpuscles seemed to be quite unaffected.

Jan. 5, 1881, or *nine* days thereafter, the coloured corpuscles seemed to have become almost globular in some cases, as they

no longer gave those optical characters, which are presented by biconcave discs. The great majority, however, were unchanged in shape. The colourless seemed unaffected.

Jan. 16, or *twenty* days after sucking, the coloured corpuscles were in much the same condition as they were a few days before, although some of them were now almost colourless, and had evidently lost their hæmoglobin, which tinged the fluid. Beautiful pink-coloured crystals or plates were now obvious—the crystals of hæmoglobin. These crystals were rectangular rhombs, while others were four-sided prisms. When the blood was sealed up and allowed to stand for some time more crystals were developed, so that there seems to be nothing peculiar in the formation of these crystals within the alimentary canal of the leech, where the removal of the hæmoglobin from the stroma takes places slowly, and the other conditions seem favourable for slow crystallisation. The twentieth day is the earliest date at which we observed these crystals to be formed from human blood. We find that Budge noticed similar crystals as early as 1850; and both he and Boganowski<sup>1</sup> observed them six or eight weeks after the sucking. Our observations show that these crystals may be formed under certain conditions as early as the twentieth day after sucking. Boganowski<sup>2</sup> observes that he found crystals resembling *hæmin* crystals fourteen days after sucking. We have not observed such crystals.

In a leech which sucked on Dec. 27, 1880, on its blood being examined on April 10, 1881, *i.e.*, three months and a half afterwards, we found plenty of crystals, and numerous blood corpuscles still undissolved. Their colour was changed to that of a dark dusky pink-like reduced hæmoglobin. These crystals can be found in the alimentary canal for a very long time after sucking. In a leech which sucked on Sept. 27, 1880, on applying a 5 per cent. solution of acetic acid the leech vomited a small quantity of a syrupy dark maroon-coloured blood, which contained large masses of crystalline pigment, quite obvious to the unaided eye. This was on Feb. 22, 1882, *i.e.*, a year and five months after sucking. The spectrum of the blood was decidedly that of reduced hæmoglobin. The crystals were very dusky red in hue,

<sup>1</sup> *Zeitschr. f. wiss. Zoolog.* xii. 332.

<sup>2</sup> *Op. cit.* p. 332.



as if there was a purplish tinge in the larger ones. They were four-sided prisms, some of them with nearly equal sides, while others were more oblong. When pressure was applied to them they tended to cleave transversely and longitudinally, so as to form many smaller prisms, or small blocks. Various forms of hæmoglobin crystals obtained from leeches are shown in Plate XII., figs. 1 and 2.

On examining the blood very carefully after it had been in the intestinal canal of a leech for one year and five months, we found that the hæmoglobin was almost all in a state of solution in the condition of reduced hæmoglobin, and outside the corpuscles. Many of the coloured corpuscles had almost entirely disappeared, but on careful examination, after the addition of water, we could obtain traces of their stroma still undissolved.

In another leech, which sucked blood on Christmas day, 1880, we still found many blood corpuscles on March 14, 1882, *i.e.*, about fifteen months since its last full meal. Much of the hæmoglobin had passed out of the corpuscles and tinged the fluid, while the coloured corpuscles which remained, and which in some samples of blood were numerous, had all a dusky dirty red hue; occasionally, what seemed to be colourless corpuscles were found. The hæmoglobin crystals were so numerous as almost to fill the field of the microscope in some parts of the preparation. We made a careful examination of the excreta of these leeches, but we never found blood corpuscles, either coloured or colourless. All we found were black masses of pigment, somewhat prismatic in shape, and also a few yellowish granules or blocks of pigment. Chlorophyll and diatoms we find pass unchanged through the intestinal canal of the leech.

We are therefore unable to say at present how the crystalline hæmoglobin and the hæmoglobin in solution within the intestine of the leech becomes transformed, in part at least, into this dark-coloured melanin-like pigment.

In another leech which sucked human blood, and which we kept for more than eighteen months, we found similar crystals. The crystals were at first dark dusky pink, but after exposure to the air, they assumed a bright light brick-red colour from the action of the oxygen upon them. This bright colour they retain for months.

PART II.—*The Formation of Crystals of Hæmoglobin.*

## BLOOD OF THE RAT.

It has long been known that the blood of many animals yields crystals of hæmoglobin with extreme ease, and amongst these the blood of the rat stands pre-eminent. If a drop of blood taken from a white rat be mixed with a few drops of water and covered with a cover glass, crystals of hæmoglobin will be found within a few minutes. This fact was demonstrated by Hoppe-Seyler in 1865. Indeed, so rapidly and certainly does this occur that the process can be demonstrated to a class of students with ease.<sup>1</sup> It is quite immaterial whether the blood is drawn fresh from the artery, whether it is defibrinated or has been allowed to coagulate, it may even be taken from the body of the animal one or two days after the death of the animal. All that is required is the addition of water in sufficient quantity. If the amount of water is large the crystals form more rapidly, and are small, and are usually arranged in the form of rosettes or stars; while, if the blood be but slightly diluted with water, the crystallisation takes place more slowly, but the crystals are larger and more deeply tinged. The shape of the larger ones is shown in Plate XII., fig. 4. In colour they are pale yellow, but when they are somewhat thicker they have a brick-red tinge. The shape of some of these crystals is shown in Plate XII., fig. 4.

We next determined to ascertain the effect of various reagents on the formation of these crystals, and first of all we tried a  $\frac{1}{2}$  per cent. solution of common salt. This merely causes crenation of the coloured corpuscles, and although we have kept the mixture sealed up for months no crystals have been developed. The common salt solution, therefore, seems to prevent the diffusion of the pigment from the stroma, or if it does not do this it prevents crystallisation. We tried *human urine*, and it had an exactly similar effect; the corpuscles became crenate, no crystals were formed, even after the preparation had been sealed up for two or three weeks. We then tried a weak solution of *pure urea*, which behaved exactly like water, only the blood corpuscles

<sup>1</sup> W. Stirling, *Text-Book of Practical Histology*, 1881.

here were rapidly dissolved, as urea is a well-known solvent of these corpuscles. The solution of the corpuscles is not quite complete, as careful observation and shading the light reveals the outlines of what appears to be the residue of the stroma in the field of the microscope. The crystallisation took place exactly as if water had been added. It is clear from these experiments that the presence of the common salt in the urine is sufficient to neutralise the effect of the urea and prevent the crystallisation. The crystals formed in the urea solution were exactly the same as those formed after the action of water.

*Effect of Chloroform.*—Chloroform was freely mixed with a drop of rat's blood on a slide; and covered and examined in the usual way. Within a few minutes crystals appeared, but the crystals, although coloured and clearly consisted of hæmoglobin, were *hexagonal*; some of them were about the size of a coloured blood corpuscle, while others were twice that size. In this case, by the addition of a different reagent to extract the hæmoglobin, a different form of crystal was obtained. It is not difficult to see how the form of crystal obtained by the action of water on rat's blood may be transformed into a hexagon, as the ends of the flattened prism are bevelled, all that is required is that the crystal be shorter. The outline of the crystal obtained from a watery solution of rat's hæmoglobin is already an irregular hexagon. The shape of the chloroform crystals is shown in Plate XII., fig. 5. These crystals we have kept for months sealed up, and they remain unchanged.

*Effect of Sulphuric Ether.*—When a drop of rat's blood was treated in the same way with ether, numerous globular masses of hæmoglobin were obtained, some of them larger than coloured corpuscles, others smaller, but it was only occasionally that a crystal completely or imperfectly hexagon was obtained.

*Effect of a Galvanic and induced Current of Electricity.*—It occurred to us that perhaps the opposite poles of a galvanic current might have an effect upon the formation of the crystals after the addition of water. On making the experiment we found that their formation went on equally well both at the negative and positive poles. The induced current had also no effect upon the process.

## BLOOD OF FISH.

We employed the blood of the common gold-fish. When several drops of water are added to a drop of blood, and the two are mixed together and sealed up as a microscopic preparation, beautiful acicular or needle-like coloured hæmoglobin crystals are developed. They reach quite across the field of the microscope. The crystals are almost identical in shape with those produced in frog's blood, but they are coloured. The ease with which the blood of certain fishes crystallises has already been noted by Funke and Kunde.

The object of this paper is to show that the natural process of digestion of blood by the leech may be used as a process for the preparation of microscopic crystals of hæmoglobin. The crystals which are obtained, especially from human blood, are often very large,  $\frac{1}{16}$  of an inch or more, and are quite visible to the naked eye. They can be kept for a great length of time. We believe these blood crystals in the leech were first observed by Budge in 1850.<sup>1</sup>

The process of digestion in the leech is an extremely slow one indeed, as many coloured corpuscles are still to be found after a sojourn of nearly eighteen months in the intestinal canal of the leech. Other experiments must determine how far the tissues of the leech are nourished by the hæmoglobin or its derivatives, or by the other constituents which occur in blood.

PART III.—*Effects of various Stimuli upon the Leech.*

We tried various plans to get the leech to disgorge its blood. It is extremely difficult to cause a leech to vomit its blood by mere pressure or squeezing, so as to force the contents of the intestinal canal through the mouth, and in fact such a process injures the animal. By far the most effective applications consist of various *chemical* stimuli. Every one is aware that common salt applied to the skin of a leech rapidly causes it to disgorge the blood it may have sucked. We found an 8 per cent. solution of common salt very effective. A few drops are applied to the skin of the leech by means of glass rod, and within a minute or

<sup>1</sup> W. Preyer, *Die Blutcrystalle*, Jena, 1871.

even less the leech after many contortions soon expresses a drop of blood from its mouth. As soon as this is done, one must be careful to place the leech in water to remove the stimulus. We also employed a solution of acetic acid varying from 1 to 5 per cent., which produces the same result within about the same time. If strong acetic acid is applied, the act of vomiting is more promptly carried out, and the amount vomited is always larger, hence we usually employed dilute solutions. After being placed in water the leech executes a series of lively wriggling and partly swimming movements, and then it subsides into its usual quiet condition. A dilute solution of sulphuric acid (2 in 1000) requires half a minute, and usually much longer to produce the act of vomiting, and we are inclined to believe that all these stimuli act more rapidly when they are applied to the ventral surface of the animal. Such a solution of sulphuric acid produces vomiting in some leeches in fifteen seconds, whilst in others the latent period may be as long as two minutes or more.

We tried the effect of galvanic current rapidly interrupted, and also the effect of induced shocks. Both forms of electricity produced vomiting after a considerable latent period.

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#### DESCRIPTION OF PLATE XII

- Fig. 1. Human hæmoglobin crystals from the alimentary canal of a leech.
- Fig. 2. Various forms of human hæmoglobin crystals from the alimentary canal of a leech.
- Fig. 3. Colourless crystals from frog's blood.
- Fig. 4. Hæmoglobin crystals produced by adding water to rat's blood.
- Fig. 5. Hæmoglobin crystals produced by adding chloroform to rat's blood.

A SPECIMEN OF SOWERBY'S WHALE (*MESOPLODON  
BIDENS*) CAPTURED IN SHETLAND. By Professor  
WILLIAM TURNER, M.B., F.R.SS. L. & E.

(Read to the Royal Society of Edinburgh, Jan. 30, 1882.)

SOWERBY'S whale is one of the rarest of the cetacea which frequent the British seas. It was first recognised as a distinct species by Mr. James Sowerby, from a specimen cast ashore in 1800 on the coast of Elgin, and named by him *Physeter bidens*.<sup>1</sup> From that time to the present no properly authenticated specimen has been obtained in Scotland, although it is not unlikely that a skull in the Museum of Science and Art in this city, a description of which I gave to this Society in 1872,<sup>2</sup> may have belonged to an animal captured in the Scottish seas. Two, if not three, specimens have been obtained on the Irish coast, but I know of no example of this whale having been caught in England. In my former communication to this Society, I referred to two specimens taken in France (Havre, Calvados), one at Ostend, one on the coast of Norway, and one on the coast of Sweden, and this completed the record of this animal so far as I could find a reference in zoological literature. I was not aware at that time that a specimen had been stranded on Nantucket Island, Massachusetts, U.S., about the year 1867, and that the cranium was in the Museum of Comparative Zoology, Harvard College.<sup>3</sup> The animal was said to be new to America.

More recently,<sup>4</sup> Professor Reinhardt of Copenhagen has described

<sup>1</sup> Sowerby's *British Miscellany of New or Rare Animals*, vol. i. p. 1, 1806.

<sup>2</sup> "On the occurrence of *Ziphius cavirostris* in the Shetland seas, and a comparison of its skull with that of Sowerby's Whale," *Trans. Roy. Soc. Edin.*, vol. xxvi.

<sup>3</sup> See reference by Prof. Agassiz, in *Proc. Boston Soc. Nat. Hist.*, Nov. 16, 1867. Mr. J. A. Allen, "Catalogue of the Mammals of Massachusetts," in *Bulletin of the Museum of Comparative Zoology at Harvard College*, vol. i. 1863-1869, p. 205. MM. Van Beneden and Gervais, *Ostéographie des Cétacés*, p. 396.

<sup>4</sup> Prof. Van Beneden referred, in *Bull. de l'Acad. Roy. de Belgique*, Février, 1880, tom. xlix., to a female cetacean captured in December 1879 at Hillion, on the west coast of France (Côtes du Nord), which, from the description sent to

and figured<sup>1</sup> a female cetacean, *Mesoplodon bidens*, which he identifies with Sowerby's whale. It was captured on the 3rd February 1880, at Hevringholm Strand, on the east coast of Jutland. As naturalists have possessed so few opportunities of seeing this animal, I append an abstract of Professor Reinhardt's account of this specimen :—

"It was 13 feet 9 inches (Danish) long, and nearly full grown. Except in a few places, nearly all the cuticle was removed, for the animal had been dead for over a month. The remains of the epidermis and the interior of the mouth were blackish. The body in front of the dorsal fin was rounded transversely, but a little behind the head it was compressed laterally, and towards the dorsal mesial line. Behind the dorsal fin the body was compressed into a sharp dorsal keel, which faded away on the upper surface of the tail. A sharp keel did not exist on the under surface, though it was also somewhat compressed. The tail did not have a mesial notch between its lateral lobes. The anteriorly converging furrows in the throat ran together in this animal similarly to what was described by Mr. W. Andrews in an Irish specimen. The external auditory meatus was large and conspicuous, its size perhaps depending on the circumstance that the epidermis was removed. The external nares were semilunar and not mesial, but more to the left, so that scarcely a third was on the right side of the mesial line. Moreover, it cut the middle line a little obliquely, so that the end of the right cornu reached a little more forwards than the left, and the want of symmetry showed itself also in the left side of the crescent, being somewhat more curved than the right. The contour of the head, when looked at from above, showed a greater convexity to the outer side of the spiracle on the left side than on the right. As is well known, the mandible of this whale possesses a pair of compressed teeth, large in the male, smaller in the female. On first seeing this specimen no teeth were recognised, for the mandibular teeth were covered and concealed by the skin. But some days later, when the integument was more shrunk, two small functionless teeth, of about the size of a pin's head, were also seen on each side of the upper jaw about to emerge from the skin, in which they were so loosely lodged that they were freely movable. The more anterior tooth was 9' 3''' from the tip of the superior maxilla, the second 5''' behind the first, and at a similar distance behind the second a third could be felt on cutting into the skin, and, more posteriorly, apparently a fourth. No small functionless teeth were seen in the mandible, though they were also probably

him, might, he thought, be either *Ziphius cavirostris* or *Mesoplodon sowerbyi*. In a letter which he has favoured me with, in reply to a query for further information, he states that, having bought the skeleton, he found it to be a *Hyperoodon*.

<sup>1</sup> Oversight over d. K. D. Vidensk. Selsk., Forhldl. 1880.

present in this specimen. The skeleton, unfortunately, was not preserved."<sup>1</sup>

On the 9th November 1881, it was reported to Professor A. W. Malm of Göteborg, that a small whale had been found dead off Vanholmen, near Marstrand, Sweden. Dr. A. H. Malm went to see the specimen, and acquired the skeleton for the Göteborg Museum.<sup>2</sup>

He reports that the animal was a male, 4500 millimetres (nearly 15 feet) long, and that it was a Sowerby's whale. It was found floating by the fisherman in a narrow creek—dead, but fresh. The animal had been partially flensed before Dr. Malm saw it. The colour was dark slate, with greyish-white irregularly scattered spots, especially on the ventral aspect. The lobes of the tail measured transversely 1200 mm. The tail was not strongly concave posteriorly, as is figured by Dumortier in the Ostend specimen, but was almost transverse, with a convexity forming an obtuse angle, and not a mesial notch between its lateral lobes. The snout tapered more rapidly from the corner of the mouth. Behind the teeth the slit of the mouth resembled a bow turned upwards, but in front it was at first like a bow turned downwards, and then it was straight to the tip of the snout. The upper lips were drawn up so that the palate sank below them, and fitted into the furrow of the mandible. The teeth projected outside the lips when the mouth was shut. Only two teeth were present, one in each mandible, but an alveolar furrow extended for about 7 mm. behind each tooth, as if for the lodgment of a smaller tooth, such as was found in the Swedish specimen previously described by Professor A. W. Malm.<sup>3</sup> The tongue was fixed to the mandible, so that only the point was free. The external nares were semilunar, concave forwards, and formed a third of a circle. Anterior to the nares, the head had a fine rounded shape. The dorsal fin pointed backwards; its posterior edge considerably falcated; its length 450 mm., its height 200 mm. The anterior limbs were very small; the anterior

<sup>1</sup> Professor Reinhardt refers in his paper to two American specimens of Sowerby's whale, the one taken at Dennis, Massachusetts, in 1869, the other at Newport, Rhode Island, in the same year. But in a letter which he has favoured me with, he informs me that he is now satisfied that these animals were not *Mesoplodon bidens* (Sow.) but *Hyperoodon rostratus*.

<sup>2</sup> Göteborg's *Naturhistoriska Museum*, Zool. Zool., *Afdelningarna*, 1882. Through the courtesy of Dr. A. H. Malm, I received an early copy of his paper, which reached me a few days after my communication was read to the Royal Society. To give sequence to the narrative, I have incorporated in the text the above analysis of Dr. Malm's paper. I am indebted for a translation of Dr. Malm's description, and of that in Professor Reinhardt's paper, to a young Swedish gentleman, one of my pupils, Mr. Arwid Kellgren.

<sup>3</sup> *Hvaldjur i Sveriges Museen*, an 1869.



edge was bent slightly back; the posterior formed at the middle a rounded projecting angle. It was almost the same size as the previous Swedish specimen, also in the Göteborg Museum, but the head was a little longer, though not quite so broad, and the teeth were lower but longer (along the mandible) than in the earlier specimen.

In September 1881, the Rev. George Gordon of Birnie, Elgin, wrote to me, that when on a visit recently to Hillswick, Shetland, he had seen the skull of a small cetacean, which he was led to think was a Sowerby's whale, and that the skeleton was in the possession of Mr John Anderson of Hillswick. I accordingly wrote to Mr Anderson to request him to present the skeleton to the Anatomical Museum of the University, and at the first opportunity he most courteously forwarded the bones to me, when the accuracy of Mr Gordon's diagnosis of the species was at once confirmed. I desire to express my thanks to Mr Gordon for having so generously given me information of the specimen, and to Mr Anderson for his liberality in presenting the skeleton to the Museum.

This whale was captured in April 1881 by Mr Thomas Anderson, who has kindly furnished me with the following particulars. He saw it struggling near the shore in the Urafirth Voe, Northmavine, on the west coast of the main island of Shetland, and his attention was directed to it by hearing at short intervals a deep groan. A rifle was then fired at it, and the animal swam into a narrow creek, where it was harpooned. It was a male, 14 feet in length. The back was dark bluish-grey or slate-coloured, becoming lighter on the sides and whitish on the belly. Grey or whitish streaks and spots, often circular, were irregularly scattered over the sides. The skin was smooth, except on the belly, which was ribbed not unlike a stocking: this ribbed appearance began near the jaw and passed back beyond the flipper. A deep crevice was between the two halves of the lower jaw, which came to a point in front, but became wider and shallower behind. The beak was elongated and pointed. The mouth slit was straight in front of the teeth in the lower jaw, but behind the teeth it was curved with the convexity upwards and backwards. The blowhole was semicircular in shape, and with a flap which seemed to close it at will. The pectoral flipper seemed to be  $1\frac{1}{2}$  or 2 feet long, but no measurement was made. A dorsal fin projected from about the middle of the back. The tail measured

3½ feet between the tips of the two lobes. The two mandibular teeth projected upwards from between the lips at the sides of the snout, even when the mouth was shut; and a bunch of barnacles, about 6 inches long, was firmly attached to each tooth. The stomach was empty, and appeared very small for the size of the animal.

It is interesting to note that Urafirth Voe, where this *Mesoplodon bidens* (Sowerby) was caught, is only 4 miles from Hamna Voe, where the specimen of *Ziphius cavirostris* was obtained which I described to this Society in 1872, and for the skull of which I am also indebted to Mr. Anderson.

The following table gives, so far as is known, the sex of the different specimens which have been captured, the localities, the dates of capture, the naturalists who have described them, and the museums where the skeletons, or part of the skeletons, are preserved:—

	Sex.	Locality where Captured.	Date.	By whom Described.	Where Preserved.
1.	Male.	Brodiehouse, Elgin, .	1800	J. Sowerby, .	Oxford.
2.	Fem.	Havre, . . . . .	1825	{ De Blainville and } Cuvier, . . . . }	Paris.
3.	Male.	Sallenelles, Calvados, .	1825	Deolongchamps, .	Caen.
4.	Fem.	Ostend, . . . . .	1835	{ Dumortier and } Van Beneden, . }	Brussels.
5.	Male.	Brandon Bay, Ireland,	1864	W. Andrews, .	Dublin.
6.	...	Norway, . . . . .	1866	Van Beneden, .	Christiania.
7.	...	{ Nantucket Island, } Mass., U.S., . . . }	1867	Agassiz and Allen,	Harvard.
8.	Male.	Skagerak, . . . . .	1869	A. W. Malm, .	Göteborg.
9.	Male.	Brandon Bay, . . . .	1870	W. Andrews, .	Dublin.
10.	Fem.	Scotland (?), . . . .	1872	W. Turner, . .	{ Museum, Science } and Art, Edin.
11.	Fem.	{ Høvringholm Strand, } Jutland, . . . . . }	1880	Reinhardt, . .	
12.	Male.	Vanholmen, Sweden, .	1881	A. H. Malm, .	Göteborg.
13.	Male.	Shetland, . . . . .	1881	W. Turner, . .	{ Univ. Museum, } Edinburgh.

The circumstances under which this animal has been seen have not been favourable for the determination of its external characters, as, before it has been examined by zoologists, it has either been much decomposed or partially or wholly flensed or dismembered. From the several fragmentary descriptions by different naturalists, I have compiled the following summary of its external appearance:—

Length in adult, 14 to 16 feet. Beak long and slender. Head swelling out considerably behind the beak. Body elongated. Back dark bluish-grey or slate-coloured, sides lighter, belly whitish.

Grey or whitish streaks and spots scattered irregularly on the sides. Dorsal fin nearer the tail than the head, falcate posteriorly. A dorsal keel in front of tail. No median notch between lobes of tail. Flipper small; both its anterior and posterior borders convex. Blowhole semilunar, concave forward, not quite symmetrical. Mouth slit straight in front, but concavo-convex further back. A pair of furrows converging in front on the under surface of the throat. A pair of laterally compressed triangular teeth protruding in the male between the lips at the side of the beak; not visible in the female. Rudimentary functionless teeth present in the gum both of the upper and lower jaws.

Of the thirteen specimens which have been captured, the whole of the skull, or a portion only, has been kept in twelve instances; but in six specimens only, including the one from Shetland, have the other bones of the skeleton been more or less perfectly preserved, viz., in the Brussels, Caen, Dublin, Göteborg, and Edinburgh University Museums. Only one skeleton has been described and figured in detail, viz., the immature female in the Brussels Museum, originally by M. Dumortier,<sup>1</sup> but subsequently and more in detail by M. Van Beneden.<sup>2</sup> Several of the crania have also been figured and described, viz., the Oxford, Paris, Caen, and Edinburgh Museum of Science and Art specimens; whilst some facts connected with the other bones of the skeleton in the Caen and Göteborg skeletons have been recorded by M. Gervais<sup>3</sup> and the Messrs. Malm.<sup>4</sup>

The animal captured in Shetland was an adult male. All the epiphysial plates were fused with the vertebral bodies, and the teeth were fully erupted. Three of the lumbar vertebræ had irregular osseous excrescences on the bodies. One lumbar spine had a ridge crossing obliquely about its middle, as if it had been fractured and repaired during life.

The end of the rostrum of the skull had unfortunately been broken off to the extent probably of 5 or 6 inches, and the anterior end of the mandible had also been broken away. Owing to this injury, I cannot give the full length of the cranium; but the skull

<sup>1</sup> *Mem. de l'Acad. Roy. de Belgique*, xii. 1839.

<sup>2</sup> *Mem. Couronnés de l'Acad. Roy. de Belgique*, Oct., xvi. 1864.

<sup>3</sup> *Ostéographie des Cétacés*, p. 397.

<sup>4</sup> *Opera citata*.

as it came into my hands was  $23\frac{1}{2}$  inches long. It is not my intention to give a detailed description of the Shetland skull, as the cranial characters of this animal have already been recorded at considerable length by MM. F. Cuvier, Van Beneden, Gervais, and myself. But as these specimens were all females, it may be useful to point out some features of difference which it presented, and I shall especially compare it with the female skull in the Museum of Science and Art, Edinburgh.

In the first instance, I give a table of comparative measurements of the two crania, expressed in feet and inches.

Table of Cranial Measurements.	Skull in <sup>1</sup> Museum of Science and Art.	Shetland, <i>Mesoplodon</i> <i>bideni</i> .
Greatest height of cranium from vertex to pterygoids .	$9\frac{1}{2}$	10
Breadth of cranium across middle of superior margin of orbits	$11\frac{1}{2}$	$10\frac{1}{2}$
Breadth of cranium between zygomatic processes of squamosals .	$11\frac{1}{2}$	$11\frac{1}{2}$
Breadth between antorbital notches . . . . .	$7\frac{3}{4}$	$7\frac{1}{2}$
Breadth of occipital condyles . . . . .	$4\frac{1}{2}$	4
Premaxillæ, greatest width behind anterior nares .	5	$4\frac{1}{2}$
Premaxillæ, least width opposite anterior nares .	$4\frac{1}{2}$	4
Premaxillæ, greatest width in front of anterior nares .	4	4
Width of anterior nares . . . . .	$1\frac{1}{2}$	$1\frac{1}{10}$
Mandible, length of ramus . . . . .	$18\frac{1}{2}$	$18\frac{1}{2}$
Mandible, length of symphysis . . . . .	$9\frac{1}{2}$	$7\frac{3}{4}$ <sup>2</sup>
Mandible, greatest vertical height of ramus . . . . .	$4\frac{1}{2}$	4

The cranial sutures were not quite so distinct as in the skull in the Museum of Science and Art. The upper borders of the rostral portions of the two premaxillæ were not so much turned inwards, and the widest interval between these borders, near the base of the beak, was  $1\frac{1}{10}$  inch, and not  $\frac{5}{8}$ th inch, as in that specimen. Instead of an open meso-rostral canal, a distinct meso-rostral bone occupied but did not entirely fill up the hollow between the premaxillæ; for this bone was divided into two not quite symmetrical halves by a mesial superior furrow, in which probably an unossified part of the meso-rostral cartilage had been lodged. This furrow was bounded behind by the anterior end of the mes-ethmoid bone, which extended

<sup>1</sup> These measurements of this skull have already been published in my Report on the Bones of the Cetacea collected by H.M.S. "Challenger," 1880.

<sup>2</sup> This mandible is imperfect.

into the base of the beak for  $1\frac{1}{2}$  inch beyond the pre-maxillary foramen, whilst in the skull in the Museum of Science and Art it did not extend more than half an inch. The pre-maxillary foramen was immediately in front of the maxillary foramen in both specimens, and the lateral border of the base of the beak was in both a sharp ridge and not grooved. In the Shetland specimen the meso-rostral bone was fused both with the vomer and the pre-maxilla, though grooves on the surface showed the line of demarcation from the premaxilla. The meso-rostral bone became attenuated anteriorly and ended in a fine point immediately behind the broken end of the beak. In the type skull in the Oxford Museum, if I may judge from a cast of that specimen, the ossification of the meso-rostral cartilage had extended very completely for nearly 6 inches in front of the mes-ethmoid, and less completely for 3 additional inches. The presence of a meso-rostral bone and of large mandibular teeth are therefore characteristics of the adult male. The vomer, where it appeared mesially on the under surface of the beak, was somewhat thicker than in the skull in the Museum of Science and Art. The tympanic bones were lost, but the left petrous bone was preserved. It closely resembled the petrous bone of *Mesoplodon layardi* figured by me in the Reports of H.M.S. "Challenger."<sup>1</sup>

The pair of mandibular teeth projected nearly 2 inches beyond the alveolus. They were triangular and laterally compressed. The anterior border sloped very obliquely forward; the posterior border was almost vertical. The surface of the fang was rough, with ridges passing obliquely downwards. The crown was smooth, terminating in a point, and was separated from the fang by a well-marked line. Each tooth was partly opposite and partly immediately behind the posterior end of the elongated symphysis. A groove in the alveolar border of the bone passed for 2 inches backwards behind the erupted tooth, and in it rudimentary denticles had at one time probably been lodged.

The discovery by Professor Reinhardt of a row of rudimentary functionless teeth on each side of the upper jaw of his specimen of *Mesoplodon bidens*, is of great interest. The New Zealand *Mesoplodon grayi*, mainly on the possession of a row of minute teeth in the upper jaw, has been made, by Dr. Von Haast, a new genus,

<sup>1</sup> Report on the Bones of Cetacea, Pl. I. fig. 5, Zoology, vol. i. 1880.

*Oulodon*; but Reinhardt's observation, as he himself has pointed out, shows that this can no longer be regarded as a ground for generic distinction between it and *Mesoplodon*.

In writing the description of the other bones of the skeleton, I have followed the method which I pursued, in framing the account of the skeleton of the *Mesoplodon layardi* for the Reports of H.M.S. "Challenger," so that the skeletons of these two animals may now be compared with each other. But it must be remembered that Layard's *Mesoplodon*, though 14 feet in length, was an immature animal, whilst the Shetland Sowerby's whale of the same length was an adult male.

SPINAL COLUMN.—Only 39 vertebræ have as yet reached me, viz, C<sub>7</sub>, D<sub>10</sub>, L<sub>11</sub>, and eleven caudals. In the Brussels skeleton there are 46 vertebræ, and the formula is C<sub>7</sub>, D<sub>10</sub>, L<sub>10</sub>, Cd<sub>19</sub>. In Professor Malm's specimen, which was adult, as the epiphyses were united with the vertebral bodies, the formula is C<sub>7</sub>, D<sub>10</sub>, L<sub>9</sub>, Cd<sub>20</sub> = 46; and in the second Göteborg skeleton, obtained by Dr. A. H. Malm, there are also 46 vertebræ, but of these only 9 are dorsal vertebræ, and consequently there are only 9 pairs of ribs. The ribs, therefore, of this whale are either 9 or 10 pairs, and the vertebral formula is 46, so that seven caudal vertebræ are missing in my specimen. In Layard's *Mesoplodon* the vertebral formula is 44 or 46, and the ribs are either 9 or 10 pairs.

The length of the macerated spine—the vertebral bodies being placed in apposition—was 9 feet 11 inches. The discs varied from half to five-eighths of an inch in thickness, so that 20 inches in addition may be allowed for them, and about 8 inches for the missing vertebræ with their discs, which would make the spine a little more than 12 feet long. In the centre of each disc was a distinct and relatively large cavity, lined by a smooth synovial-like membrane, and containing a deep yellow fluid, which in the discs between the larger vertebræ might amount to about half an ounce. The arrangement resembled what I saw in 1869 in the discs of *Balenoptera sibbaldii*.

Cervical Vertebræ.—The antero-posterior diameter of the cervical series of vertebræ was 5½ inches. The body, lamina, and spine of the axis were completely fused with the corresponding parts of the

atlas, and formed a massive bone  $5\frac{1}{2}$  inches in its greatest transverse, 5 inches in its greatest dorsi-ventral, and  $2\frac{1}{4}$  inches in its greatest antero-posterior diameter. The transverse processes of these vertebræ were not ankylosed with each other. The transverse process on each side of the atlas was single, but that of the axis consisted of a short superior and a longer inferior limb, which were not united at their outer ends. The sub-occipital groove on the anterior lamina of the atlas was converted into a foramen by a bridge of bone. The last five cervicals were all free. The bodies of the 3rd, 4th, 5th, and 6th ranged from  $2\frac{1}{4}$  to  $2\frac{3}{4}$  inches in transverse diameter, and measured about half-an-inch antero-posteriorly. In each the superior and inferior limbs of the transverse process were separated from each other externally by a wide interval, which was also the case in the Brussels skeleton; but as this skeleton was perfectly adult, it may now be stated that no foramen exists at the root of the transverse process in the cervical vertebræ of this animal. The superior limb was a stunted plate; the inferior limb was more elongated; in the 3rd and 4th vertebræ it projected slightly backwards; in the 5th and 6th it projected downwards and outwards, and in the 6th it was  $1\frac{1}{4}$  inch long. In the 3rd, 4th, and 5th vertebræ, the laminae were not united mesially, so that they had no spines. In the 6th a slender stunted spine was present; the 7th had a slender spine  $1\frac{3}{4}$  inch long; its superior transverse process was  $1\frac{1}{2}$  inch long and pointed, whilst the inferior was a mere tubercle. Its body was elongated transversely beyond the articular surfaces for the vertebræ in front and behind, and on each side it had an articular facet 1 inch by  $\frac{3}{4}$  inch for the head of the first rib. The fusion of the atlas with the axis, and the free condition of the other cervicals, is obviously a character of this animal, as it was also seen in the Brussels and in both the Göteborg skeletons.<sup>1</sup> The condylar articular surfaces of the atlas were separated below by an interval of  $\frac{4}{10}$ ths inch, whilst in the young Layards' *Mesoplodon*, the interval was less than  $\frac{2}{10}$ ths inch.

*Dorsal Vertebræ.*—In this region the bodies increased in size from before backwards. The first had a pair of rudimentary tubercles

<sup>1</sup> M. Gervais, in his account of Sowerby's whale in *Ostéographie des Cétacés*, states that the first three cervicals are fused together, but he does not say in which skeleton this has been seen.

projecting from the inferior surface of the body in series with, but smaller than, the inferior transverse tubercles of the 7th cervical. A mesial ridge appeared on the ventral surface of the 8th dorsal, as in the Brussels skeleton, which became stronger in the vertebrae behind. The laminae and spines were complete in all the dorsals, and the spines, as a rule, increased in length and massiveness from before backwards, the 1st being  $4\frac{1}{2}$  inches and the 10th  $8\frac{1}{2}$  inches long. The spines of the 1st and 2nd were almost vertical, those of the rest were inclined backwards. The articular surfaces for the heads of the 2nd, 3rd, 4th, 5th, 6th, and 7th ribs were very distinct on the anterior six dorsals at the junction of the pedicle with the posterior part of the side of the body. No articular surface for the head of the 7th rib was present on the left side of the body of the 7th dorsal, but on the right side a small articular facet was situated on a slight elevation, in series with the projecting articular process for the 8th rib, on the side of the body of the 8th dorsal. The anterior seven dorsals had each a strong transverse process, for articulation with the tubercle of a corresponding rib, projecting from the pedicle close to the anterior zygapophysis. These transverse processes projected forwards in the anterior six, and outwards in the 7th. The long axis of the articular surface on the transverse process was vertical on the 1st and 2nd dorsals, oblique on those immediately behind, and horizontally antero-posterior on the 7th. No transverse process projected from the side of the neural arch of the 8th, 9th, and 10th dorsals, but a transverse process for articulation with a corresponding rib projected from the side of the body, nearer its anterior than posterior surface. It measured half an inch in the 8th,  $2\frac{1}{2}$  inches in the 9th, and 4 inches in the 10th vertebra. Zygapophyses were present as far back as the anterior pair on the 8th dorsal. A pair of strong metapophyses projected forward from the laminae of the 10th, 9th, and 8th dorsals, to overlap the laminae of the vertebra immediately in front, and rudimentary metapophyses were present on the 7th and 6th dorsals. The 7th dorsal was the vertebra of transition.

*Lumbar Vertebrae.*—The ten lumbar were almost uniform in shape, but increased in size from before backwards; the body of the last lumbar measured 5 inches in its antero-posterior diameter, and 3.2 in its transverse. The bodies were keeled on the ventral sur-



faces. The antero-posterior diameter of each body was markedly greater than the transverse. In the more anterior vertebræ the transverse process was distinctly larger than the width of the body, but in the more posterior they were almost equal. The base of this process sprang from the anterior half of the side of the body in series with the transverse processes of the last dorsals; the processes projected outwards and a little forwards, and the free end was convex. The spines were long, laterally compressed and sloped slightly backwards. The length of the 10th lumbar spine was 10 inches. The height of the last lumbar vertebra was 15 inches. A pair of broad lamelliform metapophyses projected forwards from the anterior border of the laminae close to the root of the spine, but did not articulate with the vertebra in front; from the anterior edge of the laminae of which a pair of much smaller processes projected backwards. The neural arches sprang from the centre of the bodies.

*Caudal Vertebrae.*—Twelve only were present, seven being missing. They diminished in size from before backwards. The 1st was 14 inches high, and 10 inches between the tips of its transverse processes. In the anterior three the spines were massive, and then rapidly diminished in size to the 11th, in which the spine was a slight ridge, and the neural canal admitted only a crow quill. The transverse processes were strong in the anterior five, and then rapidly diminished in size, so that in the 9th only a faint ridge was seen. Metapophyses, which were non-articular, projected forwards from the anterior edge of the laminae of the anterior nine, but none from the posterior edge. The 1st caudal was transitional in its characters between the lumbar and caudal series, for whilst the ventral surface of the body was keeled in its anterior two-thirds, it was grooved in the posterior third, and possessed an articulation for the 1st chevron bone. The vertebræ behind the first, so far as they were present, were grooved ventrally, and on at least nine bodies articulations for chevron bones were present. Only six chevron bones reached me. No vertical foramen was present in the root of a transverse process.

*Ribs.*—Of the ten pairs of ribs only four pairs were entire, the rest had been broken or sawn across. The first was the broadest, and measured 12 inches in a straight line. The anterior seven had each a head, neck, and tubercle, but in the 1st the neck was stunted. The

head of the 1st rib articulated with the body of the 7th cervical, its tubercle with the transverse process of the 1st dorsal. From behind forwards the head articulated with the body of the vertebra in front of that, with the transverse process of which the tubercle articulated; but whilst the head of the 7th left rib articulated with the body of only the 6th dorsal vertebrae, that of the 7th right rib articulated with the body of both the 6th and 7th dorsals. The 8th, 9th, and 10th ribs articulated with the transverse processes of their corresponding vertebrae.

*Hyoïd Bone* consisted of a body and great cornua anchylosed together. It had a well-marked U form, and measured  $6\frac{1}{4}$  inches between the tips of the horns.

*Sternum*.—This bone was  $19\frac{1}{2}$  inches in length and  $6\frac{5}{8}$  inches at the broadest part, and it diminished in transverse diameter from before backwards. The ventral aspect was slightly convex, and with a faint mesial keel. Each lateral border was festooned, being most projecting where it articulated with the ribs. The sternum consisted of five segments; the 4th and 5th were anchylosed together, the others articulated with each other laterally, but in the mesial third a rounded aperture of some size was interposed, and this aperture also existed between the 4th and 5th segments. This sternum corresponded with the first Göteborg skeleton in the anchylosis of these two segments, but differed both from the Brussels skeleton, in which they were still separate, and in which the 5th was divided into two lateral halves, and from the second Göteborg skeleton, in which the sternum possessed only four segments. This sternum did not display that marked inequality in the height of the opposite sides of the bone seen in the Brussels skeleton. Articulations for five pairs of ribs were at the lateral borders, one on the manubrium and four at the junction of the sternal segments.

*Scapula*.—This was a broad triangular plate-like bone, measuring 8 inches from the glenoid fossa to the vertebral border, and  $12\frac{1}{4}$  inches from the anterior to the posterior angle. The coracoid process was  $3\frac{1}{2}$  inches long; the acromion was 4 inches long, and 1.7 inch wide, being twice as broad as the coracoid.

*Humerus*.—The epiphyses of this bone were anchylosed to the shaft. The bone was only  $5\frac{3}{4}$  inches long, and the surfaces of the shaft were flattened. The bones of the forearm and manus have not yet reached me.

A SPECIMEN OF RUDOLPHI'S WHALE (*BALÆNOPTERA BOREALIS* OR *LATICEPS*) CAPTURED  
IN THE FIRTH OF FORTH. By Professor WM.  
TURNER, M.B., F.R.SS. L. & E.

(Read to the Royal Society of Edinburgh, Feb. 20, 1882.)

IN September 1872 a whale of some magnitude was seen floundering in shallow water at Snab, Kinneil, about a mile from Bo'ness, on the Firth of Forth. Some men proceeded to the spot and fastening a rope round its tail, hauled it closer to the shore, and then killed it. I was not at home at the time, but on reading a notice of its capture in the *Scotsman* of September 26, I wrote to my assistant, the late Mr A. B. Stirling, to go and see the animal. He reported to me that it was a whalebone whale, ribbed and grooved on the belly, and he was able to make the following notes on its colour and dimensions :—

The animal was black on the back of the head and body and dorsum of the tail. The belly was pinkish white, with a shade of yellow, from chin to anus. Behind the anus it was patched with white to three feet from the tail. Behind this again the colour varied from lead grey to black, and the under as well as the upper surface of the tail was black. The breadth of the dorsum of the head at the tip of the upper lip was 2 inches; one foot further back it was 9 inches; two feet back 18 inches; and at the blowholes, 3 feet. The dorsal fin had a falcate posterior border, and its long diameter along the anterior border to the tip was 1 foot 8 inches, whilst its height was 11 inches. The flipper, when disarticulated at the shoulder, was 4 feet 10 inches in length. The breadth of the tail was 8 feet 8 inches: its anterior border was convex, its posterior border festooned. From the root of the tail to the mesial notch was 2 feet 5 inches, and the depth of the notch was 5 inches. The lower jaw projected both in front of and to the sides of the upper jaw. The length of the animal, from the tip of the beak to the end of the tail, was about 37 feet, and the girth round the belly about 15 feet.

The whale was taken possession of by the Custom House officials, and was then sold by public auction.

When I saw it a few days afterwards the blubber and baleen had been removed from the animal. The baleen consisted of numerous plates, the biggest of which were about 1 foot 3 inches in length and about 6 inches wide. They were black, striped with grey and white, and the hairs projecting from the lower free border were greyish white.

The fluted state of the belly and the presence of a dorsal fin proved it to be one of the *Balænopteridæ*, or Finner whales. It was evident, both from the colour of the whalebone and the shape of the upper jaw, the lateral borders of which were straight, that the animal was not the *Balænoptera sibbaldii*, a magnificent example of which had been stranded at Longniddry only three years before.<sup>1</sup> Its size and the colour of its baleen distinguished it also from the lesser piked whale, *Balænoptera rostrata*. The absence of yellowish and greenish tints in the plates of the whalebone threw some doubt on its being *Balænoptera musculus*, but I was not in a position, in the condition the animal then was, to discriminate between *Balænoptera musculus* and *B. borealis*. I decided, therefore, to buy the carcass, and to have the skeleton prepared for the Anatomical Museum of the University. Accordingly men were engaged to take the flesh off the bones, and to separate it into pieces of convenient size for transport into Edinburgh. I regret to say that the sternum and pelvic bones were lost amidst the masses of flesh, and could not be recovered; but with these exceptions, and that of a carpal bone, some of the smallest phalanges, and perhaps one or two chevrons, the skeleton is, I believe, perfect. As the bones of the large cetacea are, from the quantity of oil which they contain, difficult to clean, I obtained permission from my colleague, Professor Balfour, to bury them in the Botanic Gardens in a mixture of leaves and earth. They remained there until the summer of last year, when they were disinterred, clean and free from grease and smell. The examination which I have subsequently made of the skeleton has satisfied me that the animal was the cetacean named by zoologists *Balænoptera borealis* or *laticeps*.

<sup>1</sup> See my account of this animal in *Trans. Roy. Soc. Edinburgh*, 1870, vol. xxvi.

The only specimen captured in the British seas, which has been referred to this species by some zoologists, is one stranded at Charmouth, Dorsetshire, in 1840. From the colour of its baleen and the number of its vertebræ (60), it was much more likely to have been *Balænoptera musculus*. But as the skeleton has not been preserved, its identification is now rendered difficult. As no properly authenticated specimen of *B. borealis* had therefore previously been captured in the seas of our islands, I have the satisfaction of adding this mammal to the British Fauna.

In the Museum of the Royal College of Surgeons of England are some vertebræ and other bones of this species, but nothing is known of their history. In the University Museum, Cambridge, are the skull and one scapula of a whale cast ashore on the island of Islay, in 1866, which Professor Van Beneden referred at one time to this species.<sup>1</sup> Mr J. W. Clark has, however, pointed out<sup>2</sup> that it is the skull of a very fine *Balænoptera rostrata*, and Professor Flower tells me that he is of the same opinion. Skeletons of *B. borealis* are preserved in the Museums at Leyden, Berlin, Bergen, Brussels, and Bayonne. The Leyden specimen was taken in 1811, near to Moniken Dam, in the Zuider Zee, and the characters of the skeleton have been given by Professor Flower.<sup>3</sup> The Berlin specimen, figured and described by Rudolphi,<sup>4</sup> was taken in 1819 on the coast of Holstein, near to Gromitz. Two skeletons are at Bergen, the one was described by Lilljeborg,<sup>5</sup> from a young animal captured on the Norwegian coast; the other came from the Loffoden Islands,<sup>6</sup> and has apparently not yet been described. The Brussels skeleton was obtained by Eschricht from the North Cape, and has been described by Van Beneden.<sup>7</sup> The skeleton in the Museum at Bayonne is a young male specimen, which M. P. Fischer states was stranded near

<sup>1</sup> *Ostographie des Cétacés*, p. 202.

<sup>2</sup> See reference in *The Fauna of Scotland*, "Mammalia," by E. R. Alston, Glasgow, 1870. Also, in a letter to myself, in reply to a communication on the subject.

<sup>3</sup> *Proc. Zool. Soc. London*, Nov. 8, 1864.

<sup>4</sup> Rudolphi named this animal *Balaena rostrata*. *Abhand. der Akad. der Wissensch. zu Berlin*, 1820, 1821.

<sup>5</sup> Translation of Lilljeborg's *Memoir on the Scandinavian Cetacea*, in publications of Ray Society.

<sup>6</sup> *Ostographie des Cétacés*, p. 201.

<sup>7</sup> *Ibid.* p. 302.

Biarritz in July 1874.<sup>1</sup> The animal was 7·83 mètres long (about 26 feet). *B. borealis* is therefore a denizen of the North Atlantic Ocean.<sup>2</sup>

It is not my intention on this occasion to enter in detail into the description of the skeleton of this whale stranded near Bo'ness, but it will be necessary to refer to such points in its anatomy as will give certainty to the identification of the species. The bones of this animal were less massive, smoother on their surface, and less porous than the bones of either *B. musculus* or *B. sibbaldii*. The vertebral plates were not ankylosed to their respective bodies, and the epiphyses of the radius, ulna and humerus were not united to the shafts of their respective bones.

The entire length of the skull and spine was 35 feet 2½ inches, viz., the skull, 8 feet 1½ inches; the spine, without the intervertebral discs, 26 feet 7 inches. If to this be added 2 feet for the probable thickness of the intervertebral discs, and 8 or 10 inches for the projection of the lower jaw beyond the upper and the thickness of the skin, the length of the animal would have been about 38 feet, which closely approximates with what was reported as its length before being flensed. Although the condition of the epiphyses proved that the animal had not reached adult life, or probably attained its full dimensions, yet this skeleton is larger and

<sup>1</sup> *Comptes Rendus*, 27th Dec. 1876, p. 1298, vol. lxxxiii.; and *Journal de Zoologie*, vol. v. p. 462, 1876.

<sup>2</sup> In *Nature*, 12th Oct. 1876, is a reference to the *Schriften der naturforschenden Gesellschaft in Dantzig*, which contains photographs of the skeleton of a whale, said to be *Pterobalæna laticeps* (Gray), stranded in Dantzig Bay in 1874, but as I have not been able to obtain a copy of the Dantzig publication, can make no further reference to it. I observe that in the *Archiv für Naturgeschichte*, 1875, 41st year, third part, p. 338, is an elaborate description, by Professor Zaddach of Königsberg, of a female fin whale, stranded in August 1874, between Neufahrwasser, the harbour of Dantzig, and the village of Heubude. He names it *Balenoptera musculus*. Its vertebral formula is  $C_7D_{14}L_{13}Cd_{24} = 60$ , and the baleen is described as yellow like horn, with bluish green or blackish spots at the outer border. Its length was 10·98 mètres (about 36 feet English). Professor Zaddach states that he does not give a detailed description of the skeleton, as the Dantzig Society of Natural History had decided to publish a description of it, with drawings and photographs, in their *Schriften*, from the pen of Professor Menge. Can this be the specimen referred to in *Nature*? The colour of the baleen and the vertebral formula (probably the last two caudals had not been ossified and preserved) show its affinity to *B. musculus* rather than to *B. laticeps*.

more advanced in its ossification than the other skeletons which have been described. The Leyden specimen is said to have been 32 feet long, and Professor Flower gives its skeleton as 29 feet 7 inches, without the intervertebral discs, for the thickness of which an additional 2 feet should be allowed. Professor Rudolphi states that the Berlin skeleton is 31 feet 1 inch Rhenish measurement. The young skeleton in Bergen described by Lilljeborg is 30 feet 2 inches; and Professor Van Beneden names 32 feet as the length of the specimen in the Brussels Museum. From these measurements, M. Van Beneden concludes that this species of whale does not appear to exceed 35 feet in length. But the dimensions of the specimen now before us, which is obviously not an adult, would lead one to say that this animal may attain a length of 40 feet, or even more. Whilst larger than *B. rostrata*, it is considerably smaller than *B. musculus*, and still more than *B. sibbaldii*.

*Spine*.—The vertebræ numbered in this specimen fifty-six; and as the last caudal was only  $1\frac{1}{2}$  inch in its transverse and  $1\frac{1}{4}$  in its antero-posterior diameter, it is probable that all the vertebræ were secured. The formula was as follows:— $C_7D_{14}L_{14}Cd_{21}=56$ . In the Leyden specimen there were also fifty-six vertebræ, the two last caudals being fused together, viz.,  $C_7D_{13}$  or  $_{14}$ ,  $L_{16}$  or  $_{15}$ ,  $Cd_{20}$ . Rudolphi states that the Berlin skeleton had fifty-four vertebræ, though Eschricht says fifty-five, and one was probably missing. Lilljeborg states that the young skeleton in Bergen had fifty-five vertebræ, viz.,  $C_7D_{13}L_{14}Cd_{21}$ . In the Brussels specimen the terminal caudals were lost, but the vertebræ present were  $C_7D_{14}L_{15}$ , and fifteen caudals. The Bayonne skeleton had only fifty-four vertebræ, but the animal was young, and the two terminal caudals were probably not ossified. Fifty-six is probably, therefore, the normal number of vertebræ, and of these fourteen are dorsal, so that there are fourteen pairs of ribs, which is the number present in my specimen. The vertebral formula, therefore at once distinguishes *B. borealis* from the other species of the genus; for in *B. rostrata* the formula is  $C_7D_{11}L_{13}Cd_{17}=48$ , and there are only eleven pairs of ribs. In *B. musculus* the formula is  $C_7D_{15}L_{15}Cd_{25}=62$ , and there are, as a rule, fifteen pairs of ribs. In *B. sibbaldii* the vertebræ are sixty-three or sixty-four, and the formula is  $C_7D_{15}$  or  $_{16}$ ,  $L_{15}Cd_{26}$ , and there are fifteen or sixteen pairs of ribs. *Balaenoptera borealis* is,

therefore, intermediate in length, in the number of its vertebræ and of its ribs to *B. rostrata* on the one hand, and to *B. musculus* and *B. sibbaldii* on the other.

The seven cervical vertebræ of my specimen are all separate bones, except that the right lateral mass of the atlas is ankylosed to the body of the axis. This is an exceptional arrangement in the fin whales, and has not apparently been seen in any of the other skeletons of *borealis*. The atlas has a transverse process, short, twisted, and compressed from before backwards, and a median backward-projecting ventral process corresponding with Professor Flower's description of the Leyden specimen. The ring is markedly divided into an inferior part for the rudimentary odontoid, and a superior rachidian part by a process projecting inwards on each side. This character is also well seen in the atlas in the Museum of the College of Surgeons, London. The height of the atlas is  $10\frac{1}{2}$  inches; that of the axis 13 inches. The extreme transverse diameter of the atlas is  $14\frac{1}{2}$  inches; that of the axis  $21\frac{3}{4}$  inches. The axis has a thick spine. The transverse process has a slender upper and a plate-like lower limb, which unite externally to form a broad plate-like process directed backwards and outwards. The vertebrarterial foramen is only  $1\frac{3}{4}$  by  $2\frac{1}{2}$  inches, and is placed near the upper border of the process. The rachidian ring is  $4\frac{1}{4}$  by  $3\frac{1}{2}$  inches. In the 3rd and 4th vertebræ the vertebrarterial foramen is completed by the junction externally of the superior and inferior limbs of the transverse process. In the 5th vertebra these processes are separated externally by an interval of  $\frac{1}{2}$  inch; in the 6th, by an interval of  $1\frac{1}{2}$  inch; in the 7th, whilst the superior limb is long and curved downwards and outwards, the inferior limb is represented by a mere tubercle. In the Leyden specimen the vertebrarterial foramen is incompletely bounded in the 2nd to the 7th inclusive. In the Berlin and Bergen skeletons only the axis has the foramen completely bounded by bone. In the Brussels skeleton only the axis and 3rd vertebræ. The ossification is therefore more advanced in this region in my specimen than in these other skeletons.

The flat surfaces of the upper transverse processes are almost vertical; those of the lower transverse process of the 6th cervical are almost horizontal. From the root of the lower transverse process of the 4th, 5th, and 6th a stout conical process projects forwards.



The dorsal vertebræ increased in magnitude from before backwards. The 1st had a vertical diameter to the summit of the spine of  $13\frac{1}{2}$  inches, and a transverse, between the tips of the transverse processes, of  $19\frac{1}{2}$  inches. The last dorsal had a vertical diameter of  $22\frac{3}{4}$  inches, and a transverse of 30 inches. All the dorsal vertebræ were marked by an articular surface for a rib at the free end of the transverse process. They were not keeled on the ventral surface of the body.

The lumbar vertebræ were, as a rule, somewhat bigger than the hinder dorsal, and reached their maximum at the 8th, 9th, and 10th. The 9th lumbar had a vertical diameter of  $24\frac{1}{2}$  inches, and a transverse of 31 inches. Behind the 10th the transverse and spinous processes gradually diminished in their amount of projection as they passed back into the caudal region, and in the 12th vertebra from the end of the tail the transverse process was represented by a faint ridge on the side of the body, and the spine was rudimentary. In the 9th vertebra from the end of the tail the spine and laminæ had disappeared, and the neural canal was represented by a groove, which was faintly seen on the two vertebræ next behind, and then disappeared. In the 12th vertebra from the end of the tail a foramen, directed vertically, was situated at the root of the rudimentary transverse process, and a similar foramen was found in the caudals up to and including the 18th from the end of the tail. This arrangement closely resembles that figured by Rudolphi in the Berlin skeleton. The lumbar were all keeled on the ventral surface of the body, though, as a rule, the keel was slight in relation to the size of the bone.

Eleven chevron bones were present. The articulations for the chevron bones began on the 15th vertebra of the lumbo-caudal series (counting from the front) in a pair of surfaces situated at the posterior border of the ventral surface of the body of that vertebra; and distinct articulations for these bones could be seen as far back as the 27th lumbo-caudal vertebra. Hence I have regarded all the vertebræ behind the 14th lumbar as caudal vertebræ. In the 1st caudal the ventral keel was very slight, and a shallow groove was seen posteriorly between the articulations for the 1st chevron bone. The caudals behind the first were all grooved on the ventral surface, and the groove in many instances possessed considerable depth.

The spines of some of the lumbar and the more anterior caudals had sharp bony outgrowths projecting from the posterior border.

*Ribs.*—There were fourteen pairs of ribs. The 1st was flattened, and measured in a straight line  $25\frac{1}{4}$  inches on the left side and 25 inches on the right; the breadth of the sternal end of the 1st left was  $4\frac{3}{4}$  inches. The 2nd rib was  $37\frac{3}{4}$  inches long on the left side and  $33\frac{1}{4}$  inches on the right side. The ribs increased in length, as far back as the 6th rib, which measured 4 feet 3 inches in a straight line; from the 7th to the 14th they diminished in size, and the last rib was 2 feet 3 inches in length; they were slender rod-like bones. All the left ribs were separate bones, but the right 1st rib was fused with the 2nd at the sternal end, where they formed a plate of bone  $13\frac{3}{4}$  inches broad. The breadth was partly due to a broadening of the sternal end of the 2nd rib, and partly to the formation of bone in the 1st intercostal space, for a distance of 6 inches from the sternal end, which was fused with the borders of both the 1st and 2nd ribs, and, together with them, formed the broad plate above referred to. On the 3rd and 4th ribs well-marked capitular processes extended from the vertebral articular surface towards the body of the vertebra. Rudimentary capitular processes were present on the 2nd and 5th, but were absent on all the other ribs.

In all the skeletons of *B. borealis* which have previously been described, the 1st rib had the peculiarity of possessing two heads. In the Leyden specimen the cleft which separated these heads from each other had a depth of 5 inches (Flower). From the figure given by Rudolphi of this rib in the Berlin skeleton, it is probable that the cleft had a similar depth. In Lilljeborg's Bergen skeleton the 1st pair are considerably wider than the others, with the upper end forked or "biceps," and the lower rather dilated and much wider than the upper. In the Brussels skeleton, as figured by Van Beneden, one of the first ribs has the same character, and in the Museum of the Royal College of Surgeons of England are the first ribs of two individuals of this species, which are also bicipital. M. Fischer does not describe the skeleton of the Bayonne specimen, but notes that the spinal end of the 1st rib is bicipital. M. Van Beneden had the advantage of seeing the ribs in place before the flesh was removed. He states that on the right side an elongated, compressed movable

bone, curved like a rib, was applied to and articulated with the anterior surface of the 1st true rib, and on the left side a similar rudimentary rib was present, which was fused with the body of the first true rib, so as to give it a bicipital appearance. Both Professors Flower and Van Beneden regard this supplementary part of the 1st rib as a cervical rib, so that the double vertebral end of this rib articulates with both the 7th cervical and 1st dorsal vertebrae. In my skeleton no supplementary cervical rib was present. The two ribs which were fused together on the right side, at the sternal end, were the 1st and 2nd thoracic ribs, and differed therefore from the condition described in the other skeletons. The constancy with which the bicipital form of the 1st rib had been seen, in the skeletons of this animal previously described, had given rise to the impression that it might almost be regarded as a specific character, and the late Dr. J. E. Gray even went so far as to give it generic importance, and named Rudolphi's whale *Rudolphius laticeps*.<sup>1</sup> In a criticism which I made some years ago on the value of this character for purposes of classification,<sup>2</sup> I argued that the presence of a cervical rib, whether blended or not with the 1st thoracic rib, was only an individual variation, and, as cervical ribs occasionally occur in men as well as in whales, that one should as little think of classifying men who possess cervical ribs as distinct from those who do not possess them, as found a genus of whales on the presence of these bones. The absence of a cervical rib in the skeleton now described, which is an undoubted specimen of *B. borealis* or *laticeps*, shows that the bicipital form of the 1st rib is not constant in this species; but as the majority of the skeletons which have been examined have two heads to this rib, its bicipital character would seem in this animal to be the rule, and not, as in man, the exception.

*Skull.*—The skull resembled, both in its general configuration and in detail, the figures of the skulls of this animal published by Rudolphi and Van Beneden. The sides of the beak were straight; the premaxillaries projected 6 inches beyond the superior maxillaries. The upper surface of each nasal bone was  $8\frac{1}{4}$  inches long, flattened

<sup>1</sup> *Catalogue of Seals and Whales, and Supplement.*

<sup>2</sup> The so-called two-headed ribs in Whales and in Man, *Journal of Anat. and Phys.* vol. v.

in the posterior third, but slightly concave in the anterior two-thirds, owing to the inner border of that part of the bone being raised into a low longitudinal ridge. The anterior borders of the two bones were truncated, and formed together almost a straight line. The breadth of each bone at its posterior end was 1 inch, and at its anterior end  $2\frac{1}{2}$  inches. The premaxilla passed back as a thin plate between the nasal and superior maxilla, as far as the posterior end of the nasal. The superior maxilla passed back for 3 inches beyond the posterior end of the nasal.

The orbital border of the frontal bone was 14 inches in length; the antero-posterior diameter of the inner part of this bone was 16 inches. The anterior borders of this bone sloped outwards and slightly backwards, whilst the posterior border was almost transverse.

The anterior border of the occipital bone was  $10\frac{1}{2}$  inches wide, and almost transverse; the posterior border was  $31\frac{3}{4}$  inches, and presented on each side from the foramen magnum outwards two concavities separated by an intermediate convexity.

The malar was 12 inches long, inclusive of the thin plate between the lachrymal and superior maxilla; the part which formed the proper lower boundary of the orbit was 8 inches. The lachrymal,  $8\frac{1}{2}$  inches long, was a thin plate of bone except at the anterior end, which was tuberculated.

The beak arched in the antero-posterior direction from base to tip, and the highest point of this arch was 5 inches above the chord of the arc.

The mandible was not strongly curved. The length of the lower jaw in a straight line was 8 feet  $5\frac{1}{4}$  inches; and along the outer convex surface 8 feet 8 inches. The superior border was 11 inches at its farthest point from the chord of the arc. The coronoid process was low and triangular, its base was  $8\frac{1}{2}$  inches long, and its height from the base line was  $3\frac{1}{2}$  inches. The depth of the mandible, including the coronoid process, was  $11\frac{3}{4}$  inches.

The hyoid closely resembles Rudolphi's figure, and, as it has not hitherto been properly described, and is so characteristic of this species, I shall give an account of it. The inferior surface of the middle of the body was slightly concave from side to side, and slightly convex from before backwards. The posterior border of

the body was convex, and projected backwards considerably behind a line drawn across between the posterior border of the two great cornua. In this character it differed very markedly from both *B. sibbaldii* and *B. musculus*. The great cornu was flattened on its upper and slightly convex on its under surface, with the anterior border a little more rounded than the posterior; its antero-posterior diameter was almost uniform until near the tip. In *B. sibbaldii*, again, the great cornu swelled out so as to assume a remarkable fusiform shape, and to a less degree this is also seen in *B. musculus*. The stylo-hyals were flattened on both surfaces and  $13\frac{1}{2}$  inches long. The hyoid measured between the tips of the great cornua 21 inches.

The following table gives, in feet and inches, the principal dimensions of the skull:—

	FT.	IN.
From anterior border of foramen magnum over vertex to tip of beak . . . . .	8	$9\frac{1}{2}$
From nasal end of superior maxilla to tip of beak . . . . .	6	9
From anterior border of foramen magnum to nasal end of superior maxilla . . . . .	2	$0\frac{1}{2}$
Breadth across posterior ends of superior maxillæ . . . . .	0	$8\frac{1}{2}$
Greatest breadth of skull . . . . .	8	$10\frac{1}{2}$
Breadth of skull at base of beak . . . . .	2	6
Breadth of skull at middle of beak . . . . .	1	$5\frac{1}{2}$
Breadth of skull between middle of orbital borders of frontals . . . . .	3	$7\frac{1}{2}$
Length of skull in a straight line (condylo-premaxillary) . . . . .	8	$7\frac{1}{2}$
Length of beak . . . . .	5	9
Length of superior maxilla . . . . .	6	3
Length of premaxilla . . . . .	6	$6\frac{1}{2}$
Greatest breadth of superior maxilla behind base of beak . . . . .	3	7
Greatest breadth between outer borders of both premaxillæ . . . . .	0	10
Greatest breadth of space between both premaxillæ . . . . .	0	8
From foramen magnum to upper border of occiput . . . . .	2	0

I have compared the tympanic bones with those of *B. rostrata*—the smallest—and *B. sibbaldii*—the largest—of the fin whales. Their dimensions expressed in inches were as follows:—

	Immature. <i>B. rostrata</i> .	Adolescent. <i>B. borealis</i> .	Immature. <i>B. Sibbaldii</i> .	Adolescent. <i>B. Sibbaldii</i> .
Length, .	3.5	4.8	4.7	5.3
Breadth, .	1.8	2.2	2.5	2.6
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In *B. rostrata* the breadth index was 51, in *borealis* 46. The greater relative breadth of *rostrata* was due to both the outer and inner surfaces being generally more convex; but the anterior part of the outer surface of *rostrata* possessed a concave area more distinctly marked than in *borealis*. In *rostrata* the superior border between the two attachments of the petrosal was more horizontal than in *borealis*, and in the same specimen the posterior attachment of the petrosal was nearer the hinder end of the bone, which was more rounded in *rostrata* than in *borealis*. In *rostrata* the entrance to the tympanic cavity was relatively wider, with the rounded border relatively thicker; whilst the inferior border was less distinctly keeled than in *borealis*. In the adolescent *B. sibbaldii* the tympanic was broader in relation to the length than in *borealis*, and this greater relative breadth was still more strongly marked in the immature *sibbaldii*, the breadth indices in the two specimens being respectively 49 and 53. The greater breadth of *sibbaldii* was especially seen in the posterior third of the bone, which bulged considerably on the inner surface in *sibbaldii*, and was almost flat in *borealis*. This bulging caused a broadish concave surface at the root of the posterior attachment of the petrous bone in *sibbaldii*, whilst the corresponding area in *borealis* was a narrow groove. In *sibbaldii* the entrance to the tympanic hollow was more sinuous, and the rounded border of the bone much thicker than in *borealis*. In *sibbaldii* the anterior and posterior ends were less attenuated than in *borealis*. Through these characters there was no difficulty in distinguishing between the tympanics of these species, and the character which most impressed me in making this comparison was the relative want of breadth of *borealis*. I have not a tympanic bone of *B. musculus* at hand with which to compare my specimen, but from my recollection of a specimen I saw in the Royal Museum, Brussels, in May of last year, I should say that in *musculus* the tympanic had a greater breadth than in *borealis*.

*Pectoral Limb.*—The scapula was a triangular plate, measuring 29 inches between its superior and inferior angles, and  $17\frac{1}{2}$  inches in its glenoido-vertebral diameter. The coracoid was  $\frac{3}{4}$  inch long, and  $2\frac{1}{4}$  in its greatest breadth. The acromion was  $6\frac{3}{4}$  inches long, and 4 in its greatest breadth. The humerus was only 11 inches long, and 1 foot  $2\frac{1}{4}$  inches in circumference at the middle of

the shaft, the surfaces of which were somewhat flattened. The ulna was 11 inches long,  $5\frac{1}{2}$  broad at the upper end, measured across the olecranon,  $3\frac{1}{2}$  at the lower end, and only  $2\frac{1}{4}$  in breadth in the middle of the shaft. The radius was  $20\frac{1}{2}$  inches in length, and with much less difference in breadth between the upper and lower ends and the middle of the shaft than was seen in the ulna.

Thirteen carpal bones were present, and one was probably missing. Five pairs of these bones were nodular, in part tuberculated on the surface, and in part with flattened articular-looking surfaces, well marked in four pairs, very imperfect in one pair. One pair and the single bone were flattened, elongated, and uniformly tuberculated, without any appearance of an articular surface.

Thirty-three finger bones were present, and some of the smaller phalanges were either unossified or probably missing.

The comparison which I have so far made of the Bo'ness whale with other specimens has been with skeletons admittedly those of *B. borealis*, and obtained in the North Atlantic. But in the Leyden Museum is a skeleton brought from the north-west coast of Java, named, after Professor Schlegel, *Balænoptera Schlegelii*, which Professor Flower, who first described it, regarded as closely allied to, if not specifically identical with, *B. borealis*, though, on account of its *habitat*, he had a difficulty in placing it with *borealis*.<sup>1</sup> This skeleton was more perfectly ossified than in my specimen, and belonged to an animal probably about 45 feet long. I have compared my skeleton with Professor Flower's description and figures, and with the additional description and illustrations of its skeleton in Pl. XIV. and XV., and on p. 221 of the *Ostéographie des Cétacés*, and without doubt the resemblance is in many particulars very striking.

The mandible with its low coronoid process, the profile outline of the skull, the flattened stylo-hyals, the general form of the tympanie, the cervical vertebræ, the general form of the dorsals and lumbaræ (the latter with their spiculated spines), the scapula, the humerus, radius and ulna, all closely corresponded. The Java specimen possessed also fourteen pairs of ribs and fifty-four vertebræ,  $C_7 D_{14} L_{14} Cd_{10}$ . Professor Flower thinks that three or four

<sup>1</sup> *Proc. Zool. Soc.*, London, Nov. 8, 1864.

caudals are wanting, but Professor Van Beneden considers only one or two are missing.

On the other hand, the nasal bones are obviously flatter and longer; the antero-posterior diameter of the orbital border of the frontal is relatively not so long; the double curve of the posterior border of the occipital is not so marked, and its base is wider (Van Beneden) in *Schlegelii* than in *borealis*. Again, my specimen does not have a pair of strong tubercles projecting from the posterior border of the body of the hyoid, as in the Java specimen, and the first rib is not bicipital.

Some of these differences may be due to the more advanced ossification of the Java skeleton, and the longer and stronger spines of the dorsal and lumbar vertebrae, which M. Van Beneden refers to, are obviously due to the same cause. The greater length of the Java skeleton has not the importance which Van Beneden attaches to it, as I have already maintained that *borealis* may attain a length of 40 feet or upwards, and the ribs in the Java specimen corresponded in number with those in my skeleton.

At the time when Professor Flower wrote his description, there was a greater tendency, on the part of cetologists, to limit the area of distribution of the individual species of cetacea, than now exists, and to confer specific value upon specimens which, though in many respects similar in characters, yet came from distant seas. The wider range of distribution of some of the species of the marine mammals is now more generally recognised, and the remoteness of the *habitat* of Schlegel's *Balaenoptera* ought not, if the anatomical arrangements correspond, to bar its association with *B. borealis*.



NOTE ON THE ANATOMY OF THE THYRO-ARYTENOID  
MUSCLE IN THE HUMAN LARYNX. By S. G. SHATTOCK,  
*Curator, Museum, University College, London.*

IN 1855 Manuel Garcia<sup>1</sup> recorded an observation on the anatomy of the thyro-arytenoid muscle, which in his own words was as follows:—  
“If we cut away the horizontal bundle<sup>2</sup> in successive layers, we see that the fibres are not all of the same length. . . . As the fibres all begin from the arytenoid, and terminate successively at more distant points of the crico-thyroid membrane, we see that the muscle is thicker behind than before. . . . The remarkable arrangement of the fibres which we have just examined enables us to explain a fundamental fact—the elevation of the voice. The fibres of the horizontal bundle being placed over each other in layers, one covering the other, and getting longer and longer as they become more external, extend their action to the more anterior edge of the glottis,” &c.

Ludwig<sup>3</sup> in 1857, quoting portions of this paper, similarly describes the anatomy of the muscle, and names the particular portion which terminates on the cord the “ary-vocalis.” Both Henle<sup>4</sup> and Luschka<sup>5</sup> refer to the description of Ludwig’s, Luschka asserting, however, that the muscular fibres extend for the entire length of the vocal ligament, from the thyroid to the arytenoid cartilage.

At M. Garcia’s request, I made a careful dissection to test if possible the accuracy of his own description, which I am able to confirm.

The dissection is best conducted on one of the halves of the bisected larynx. The ala of the thyroid being removed, except for a strip by the middle line, and the inner division of the thyro-arytenoid made tense, by fixing the parts on wax, the muscle may be dissected with entomological forceps beneath spirit.

The parts being dissected from the outer side, those fibres are first demonstrated which pass from the arytenoid cartilage directly across to the thyroid. The same dissection will display fibres attached to the lateral and lower portion of the crico-thyroid membrane, which lie in continuous series with the fibres of the crico-arytenoideus lateralis and are continued above into the horizontal fasciculi of the thyro-arytenoideus internus, of which they may be regarded as a portion. On teasing the fibres from the thyroid cartilage, a deeper sheet is exposed, of which the elements terminate in distinct tendons which reach the thyroid; and as these are removed, deeper sets will be found to terminate in the vocal cord and posterior portion of the crico-thyroid membrane, at shorter distances the more deeply they are placed, this differential insertion being very distinct on the vocal cord, in which the deepest set of muscular fibres are firmly inserted in front of the processus vocalis of the arytenoid cartilage.

<sup>1</sup> *Proc. Roy. Soc.* vol. vii. p. 405.

<sup>2</sup> Thyro-arytenoideus internus of Henle.

<sup>3</sup> *Physiologie*, p. 567.

<sup>4</sup> *Anatomic*, vol. ii. p. 253.

<sup>5</sup> *Der Kehlkopf des Menschen*, 1871, p. 120.

## Physiological Notices.

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REPORT OF OBSERVATIONS MADE BY PROFESSOR H. KRONECKER  
AND HERN. F. FALK AND S. MELTZER ON THE MECHANISM  
AND INNERVATION OF DEGLUTITION. By J. MCGREGOR-  
ROBERTSON, M.A., M.B., *Muirhead-Demonstrator of Physiology*  
*in Glasgow University.*

WHEN working last year in the Physiological Institute of Berlin, I had the opportunity of witnessing several interesting experiments performed by Professor Kronecker and Mr. Meltzer, in their research on the Mechanism, &c., of Swallowing. Some of the results obtained were so remarkable, and varied so considerably from generally accepted views on the subject, that I willingly undertook, with Dr. Kronecker's sanction, to put together the results he had communicated, at various times, to the Berlin Physiological Society in the shape of an article to one of the English journals of physiology. In this paper I have, therefore, collected the facts and conclusions contained in several communications from the Berlin laboratory, the references being given in the proper place.

Falk and Kronecker<sup>1</sup> first pointed out that the doctrine, that the portion of food or drink, which had reached the pharynx, was conducted thence into the stomach by peristaltic action, was insufficient. The peristaltic action alone would not account for the speed with which, *e.g.*, a draught of water passes from the mouth to the stomach. They compare the pharynx to the barrel of a syringe, the piston of which is the root of the tongue. In the normal act of swallowing the space becomes air-tight, whereupon by the quick contraction of the striated muscles the bolus or draught is forced in the direction of least resistance, *i.e.*, down the flaccid cesophagus. Where, owing to the size of the draught, the resistance presented by the cesophagus is reduced to a minimum, the draught may actually reach the stomach before the peristaltic action could make itself effective.

To prove the analogy to the action of a syringe, a water manometer was connected by indiarubber tubing with a bent glass tube. The open end of the glass tube was passed under the velum to the root of the tongue. On swallowing there occurred an increase of the pressure in the manometer, followed by a decrease, the latter corresponding to the return of the root of the tongue to its original position. If the glass tube be provided with two open ends, one communicating with the atmosphere, no variation in pressure occurs on the act of swallowing,

<sup>1</sup> *Verhand. der Physiol. Gesell. zu Berlin*, No. 13 (1879-80), S. 82.

because the compressed air escapes, and fluid cannot now be swallowed in the ordinary way. If the end open to the air be stopped, the variation of pressure returns, and swallowing again becomes easy. Exactly similar results were obtained in dogs. The attempt was made on dogs to show that the chief factor in this compression action was the tongue, and the tongue was paralysed by section of the hypoglossi. Swallowing was still possible, though evidently under difficulties. It was observed, however, that the tongue in this case was passively pressed upwards by elevation of the larynx. When this was prevented by mechanically fixing the larynx, water poured into the mouth could no longer be swallowed. For these reasons the following law was formulated:—"That the act of swallowing proper, at least in men and dogs, is effected by the quick contraction of striated muscles."

The next point to determine was whether, by experiments, the relation between the action of the striated muscles and peristalsis could be shown. After some trouble, Herr Meltzer was able to settle this by experiments performed on himself.<sup>1</sup> An œsophageal tube was taken, to which, at different distances, were attached two air capsules, on the same principle as Marey's heart sound. These capsules were brought into communication with a Marey's tambour, from which a marker projected on to a kymographion cylinder, rotating at a moderate rate. The tube was then introduced by Herr Meltzer into his œsophagus, in such a way that the first air capsule was within the region of the constrictors, while the second was further down—deeper in the œsophagus. A draught of water was then swallowed. As it passed each capsule it compressed the air and caused a mark to be made on the rotating cylinder. Two marks, consequently, were made, one signifying the moment the water passed capsule 1, the other signifying the moment the water passed capsule 2. It was found that these marks followed one another so quickly, that on a slowly-moving cylinder their time difference could not be estimated, but it could not be greater than  $\frac{1}{20}$ th of a second. After several seconds a second mark followed the first, caused by compression of the air in the *deeply placed* capsule. The mark was such as to indicate that the compression was slowly developed and lasted longer. It was evidently due to the peristalsis. The further down into the œsophagus the second capsule was removed from the first, placed in the constrictor region, the longer the mark due to peristalsis took to show itself, while the time of appearance of the mark due to the passage of the water was not sensibly increased. The peristaltic mark also became larger the more deeply the capsule was introduced; that is to say, the pressure became greater in the deep portions of the œsophagus. The pressure was specially noticed to increase when the capsule had been forced beyond the limit of the first third of the length of the œsophagus. HHrn. Kronecker and Meltzer, therefore, believe that at this point between the first and second thirds of the length of the gullet the transition takes place from striated to unstriated muscle.

<sup>1</sup> *Op. cit.* No. 18 (1880), S. 104.

It thus appeared that a draught of water passed down the œsophagus in a small fraction of a second, while the peristaltic movement took several seconds to traverse the same distance, the water reaching the stomach in about 0·1 second, and the peristalsis taking about seven seconds. That the water passes so quickly was verified in the following way:—A piece of litmus paper was pushed into an ordinary œsophageal tube till it appeared at the side openings near the blind end, the paper being fastened to a thread passing down the tube. The tube was then introduced into the gullet so far that it took the peristaltic wave five seconds from the beginning of swallowing to reach the end of the tube. A draught of vinegar was then taken, and, in not more than half a second after swallowing, the litmus paper was pulled out by the thread. The paper was distinctly red, showing that in the half second the vinegar had passed as far down as the situation of the paper. In the further observations the double capsule was discarded, and an œsophageal tube was used, which had a small fine caoutchouc ball tied over the blind end, and which was connected as before with a Marey's tambour and recording cylinder. By employing the simpler form of instrument, Herr Meltzer<sup>1</sup> found that in the uppermost portion of his œsophagus the peristaltic contraction did not appear till 1 second after the beginning of deglutition, lasting there 2 to 3 seconds; it had not reached the middle portion of the gullet for about 3·5 seconds, where it lasted 5 to 8 seconds, arriving in the neighbourhood of the diaphragm after from 5·5 to 7 seconds, and lasting 8 to 9 seconds. The speed of propagation of the peristaltic waves thus varies in different regions of the œsophagus. Mosso<sup>2</sup> has shown that the waves can be propagated even though a complete ring be cut out of the gullet, probably because the irritation is conducted in nerve tracts lying outside its walls.

Kronecker and Meltzer deem it, therefore, probable that there are four heaps of nerve ganglia, the uppermost of which releases the first act of swallowing, and the lowermost three of which are capable of being excited, *e.g.*, by eructation, without this first act.

The most interesting of the experiments performed in the research were those showing the nature of the innervation of deglutition. The animal (a dog or rabbit) having been narcotised, the superior laryngeal and glosso-pharyngeal nerves were exposed and laid over electrodes from Du Bois' inductorium, a dissection at the same time being made to show clearly the larynx and the œsophagus. On stimulation of the laryngeal nerve the larynx was pulled up as in the first act of swallowing, this being shortly afterwards followed by a peristaltic wave passing down the œsophagus. When, however, the laryngeal and glosso-pharyngeal nerves were simultaneously irritated, no such movements were seen to occur; that is to say, the stimulation of the glosso-pharyngeal inhibited the movement of deglutition that is produced by irritation of the superior laryngeal. More than

<sup>1</sup> Ueber den Schluckmechanismus und dessen nervöse Hemmung. Der Monatsbericht der Königl. Akad. der Wissensch. zu Berlin, 24th Jan. 1881.

<sup>2</sup> Moleschott's Untersuchungen zur Naturlehre des Menschen und der Thiere. Bd. xi. heft 4, No. xvii.

that, if the laryngeal be stimulated, and if thereafter, as soon as the upward movement of the larynx is observed, a stimulus be applied to the glosso-pharyngeal, the movement of the œsophagus is restrained; that is to say, the glosso-pharyngeal is capable of inhibiting the movements of deglutition even after the first stage of these movements is initiated.

This inhibitory arrangement Herr Meltzer showed in another way by experiments performed on himself. Having introduced the œsophageal tube with its transmitting ball, he performed a series of acts of swallowing quickly, one after the other, by drinking a glass of water. The registering apparatus showed no indication of the peristaltic wave until the last act of the series had been performed, and the time of the appearance of the wave was the same as it would have been had this last act been the only one. In fact one act of swallowing had inhibited the peristalsis of the immediately preceding one. The contraction, however, following the first act could not be cut short if it had already made its appearance before the second act of swallowing occurred. But the wave following the second act did not appear earlier than it would have done had that act been performed not until the first act, and its consequent peristalsis, had been completely accomplished.

Complete section of the glosso-pharyngeal causes the œsophagus to fall into tonic spasm, which may last longer than one day.

The act of swallowing was found to have an influence also on respiration, the heart, and blood pressure.<sup>1</sup>

If one holds his breath as long as possible, till in fact the desire to breathe becomes irresistible, and then, without having drawn breath, proceeds to drink a glass of water, he will find all desire to breathe passes completely away during deglutition. As soon as the swallowing is stopped, a prolonged and deep inspiration follows; and if the pulse be observed it will be found that its rapidity is greatly increased during the series of swallowing acts. During each act the blood pressure falls in the aortic system. The explanation offered is that inhibitory influences reach the medulla through the glosso-pharyngeal, and extend to the vagus fibres which excite inspiration, and to fibres of the same nerve which inhibit the movement of the heart, as well as to the vasomotor centre. The main points shown by this research are, therefore, these:—

1. Deglutition is effected by the quick contraction of striated muscles, a draught of water reaching the stomach even before the peristalsis appears.

2. The glosso-pharyngeal exercises an inhibitory influence on deglutition.

3. The movements of deglutition diminish the need of respiration.

4. Movements of deglutition increase the activity of the heart by restraining the action of the vagus; they also inhibit the action of the vasomotor centre.

<sup>1</sup> See a paper, translated by me for Prof. Kronecker, in the *Proceedings of the Royal Society*, London, vol. xxxiii. No. 216, "On the Propagation of Inhibitory Excitation in the Medulla Oblongata."

## THE SPLENIC SYSTOLE.

DR. C. S. ROY (*Journal of Physiology*, vol. iii. No. 3), in a series of experiments made in the Cambridge Physiological Laboratory with an instrument (the oncometer, or bulk measurer), invented by him, has tested the changes in volume which take place in certain organs under varying conditions. He first found that an excessively minute change in the rapidity of the urinary secretion was invariably accompanied by an increase in the calibre of the renal vessels, causing a very evident rise in the line which represented the volume of the kidney. He finds that the splenic circulation differs in one fundamental particular from that of other organs—in that it is continuously expanding and contracting with a rhythm which is remarkably constant; the time, in the dog and cat, which elapses from the commencement of the systole to end of the succeeding diastole being about 60 seconds, the duration of the diastole somewhat exceeding that of the systole. The extent of the contractions and the amount of blood forced out of the spleen at each contraction are subject to great variations, which are due, in part at least, to variations in the force of the contractions of the smooth muscular fibres which are found in the splenic capsule and trabeculæ. Stimulation of the splanchnic nerves and of the vagi or of the peripheral ends of these nerves after division of them caused contraction of the spleen; but the division of either of these nerves on one or both sides does not cause an arrest of the specific splenic contractions, nor does any expansion of the spleen result on these nerves being severed.

"In concluding, I may refer to some of the principal facts contained in the foregoing pages.

"The circulation through the spleen differs from that of other organs in the important particular that the force which impels the blood through the organ is not that of the blood pressure in the arteries. The splenic circulation is carried on chiefly, if not exclusively, by a rhythmic contraction of the muscles contained in the capsule and trabeculæ of the organ.

"The rhythmic contraction is exceedingly regular in so far as the rapidity of the rhythm is concerned, varying as it does in any given individual, but very slightly even during an experiment lasting many hours, and in which the condition of the animal has necessarily changed considerably. Roughly speaking, each contraction with the succeeding expansion lasts about one minute in the case of dogs and cats.

"As has also been pointed out, changes in the arterial blood pressure have comparatively little influence on the volume of the spleen, from which it may be concluded that the passages by which the arterial blood enters the substance of the organ are relatively very narrow, and that the pressure of the blood contained in the pulp of the spleen is not so closely connected with that of the arterial blood pressure as would be the case did the latter play a predominating part in carrying on the circulation through the organ.

"The rhythmic contraction and expansion of the spleen is different

in nature from the rhythmic contraction and expansion which may be observed in various organs on the 'Traube-Hering' blood-pressure curves showing themselves. The spleen also takes part in the production of the 'Traube-Hering' curves of the blood pressure, contracting with each rise, and expanding with each fall of the arterial pressure, but these contractions are readily distinguishable from those which are proper to the spleen, and which are independent of changes in the blood pressure. Very frequently the combination of the Traube-Hering contractions of the spleen and the 'specific splenic' contractions results in an 'interference' curve being described by the instrument which records graphically the changes in volume of the organ.

"Stimulation either of the central end of a cut sensory nerve or of the medulla oblongata causes a rapid contraction of the spleen. The paths by which such vaso-constrictor influences may travel from the cerebro-spinal centres are various. As has been shown, stimulation of the peripheral ends of both splanchnics and of both vagi causes a rapid contraction of the spleen. After section of these four nerves (the vagi in the neck and the splanchnics at their point of passage through the diaphragm), stimulation of a sensory nerve still causes a contraction of the spleen, showing that vaso-constrictor influences may pass from the cerebro-spinal centres to the spleen by some other route or routes than by the nerves named. It is not my intention, however, to give a resumé of the contents of the foregoing pages, and I will only remark, in conclusion, that the fact that section of the principal nerves which convey vasomotor influences from the cerebro-spinal centres to the spleen has so little effect on the rhythmic contractions and expansions of the organ, would seem to indicate that these latter are regulated and maintained by some mechanism contained in the spleen itself."

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### THE RELATION OF BRAIN TO MIND.

In a lecture delivered to the Dialectic Society of the University of Glasgow, Professor Cleland has discussed the relations of brain to mind. We extract from this interesting lecture the following passage:—

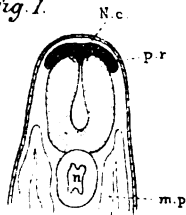
"Has, then, the energy, the conservation of which has become one of the greatest laws for the physicist, a larger circuit here than the mere material universe? That is a startling question to present itself; but once presented, I see no escape from answering it in the affirmative. It does not seem to have generally occurred to thinkers to track sufficiently in detail the steps, physical and mental, from the stimulation of a sense-organ to the operations of mind and brain consequent on sensation, so as to see that there is a transition-point where such a question is inevitable. But the inexorable question having been raised, there are only two alternatives:—at this transition-point either the physical results of the action in the nerve-cells amount to the same sum as they would in the case of the same action in cells

unconnected with the mind, or they amount to a smaller sum; if to the same sum, then the change in the mind is a something which, while brought about by physical energy, is yet additional to the constant quantity, and to that extent transgresses the physicist's law in a region external to his domain; if to a smaller sum, then the difference is transformed from physical to psychical energy, and the bridge looms vaguely between the physicist's and that other territory. But it is difficult to imagine that the former alternative can be true, seeing that at any moment, without change of external circumstances, the volition can initiate physical operations leading to movements of the body, and similarly can stop the same, and must, therefore, start the brain-changes which are its own necessary accompaniment. According to the reading of the mechanism of sensation to which this argument forces me, and which I put forward with all the diffidence which so startling a result suggests, *a certain minute amount of energy in the production of sensation quits the physical for the psychical world, instantaneously to return again in the excitation effected by the sentient mind on the substance of the brain.* While, as already said, the statement that thought is a form of physical energy cannot possibly convey any meaning, it is not only possible, but apparently necessary to admit that thought and physical energy are mutually convertible; nor will this statement long continue after all, perhaps, to wear a startling aspect to those who grasp that Spirit is the one substratum of everything.

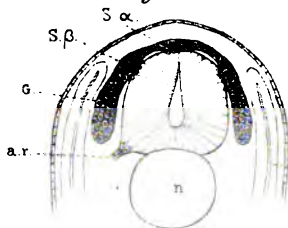
"It must also be observed that as soon as it is appreciated that mental action is either prior or subsequent to the physical change with which it is immediately associated, the important admission must follow that there is an element of mental existence independent of the body, namely, that on which the nerve-change acts in the case of sensation, and from which that volition comes whose action is linked with the brain. In that sphere, and not in the shape of potential vibrations laid past in nerve-cells, I believe it is that latent memories are stored; and I know not how much else there may be within it."



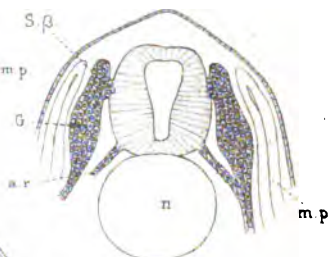
*Fig. 1.*



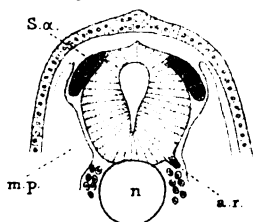
*Fig. 3.*



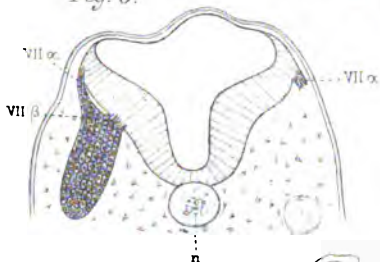
*Fig. 4.*



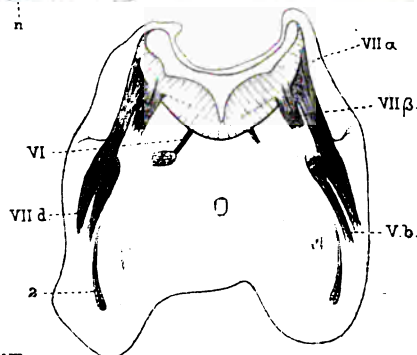
*Fig. 2.*



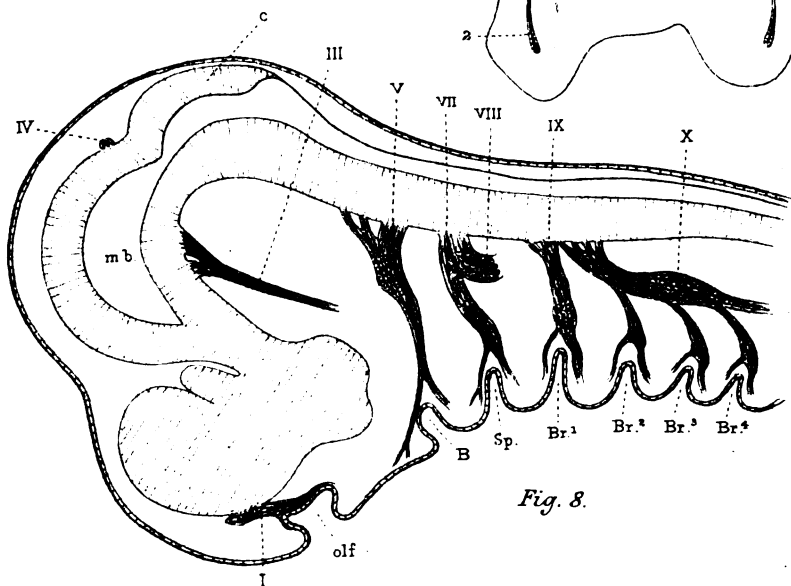
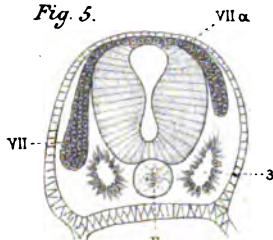
*Fig. 6.*



*Fig. 7.*



*Fig. 5.*



*Fig. 8.*



*Fig. 7.*



*Fig. 8.*



*Fig. 9.*

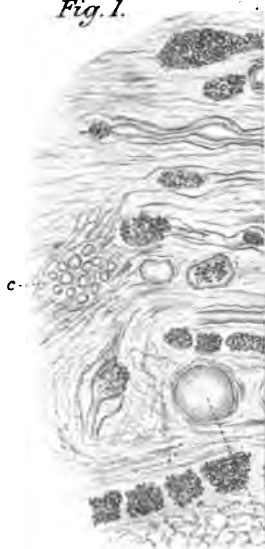


*Fig. 10.*



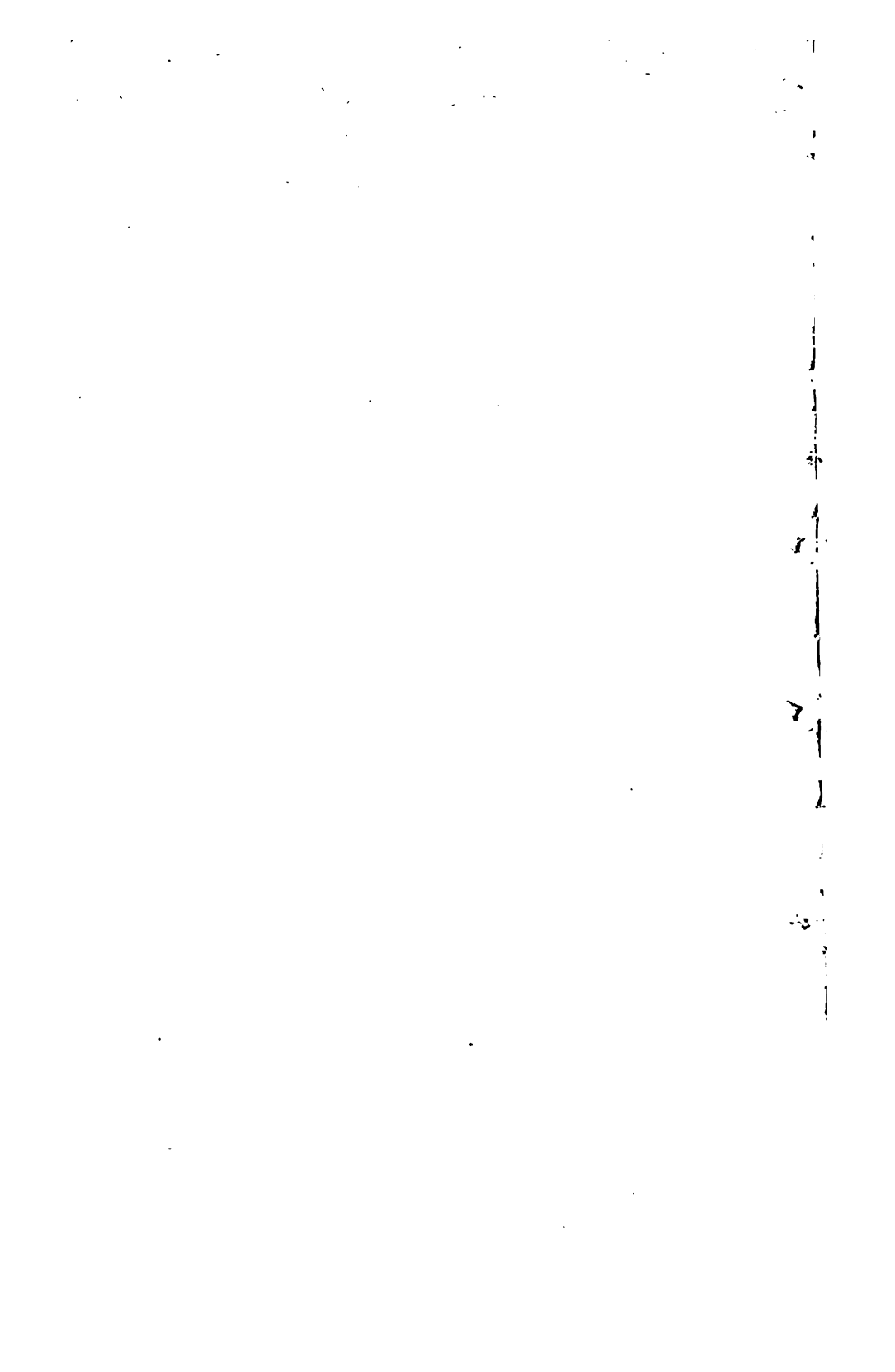
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*Fig. 1.*

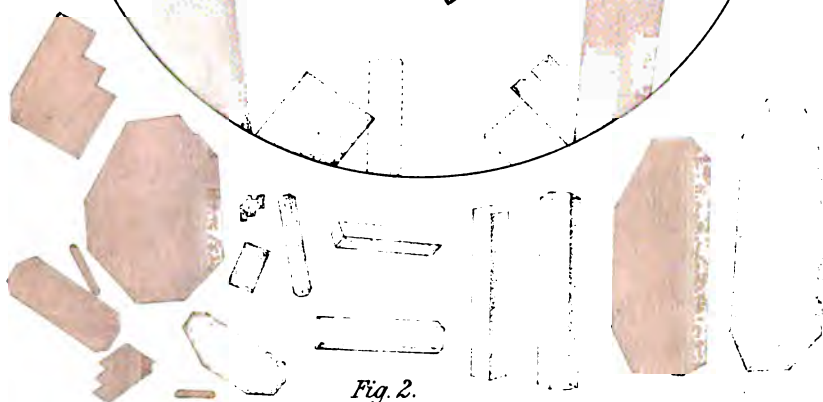
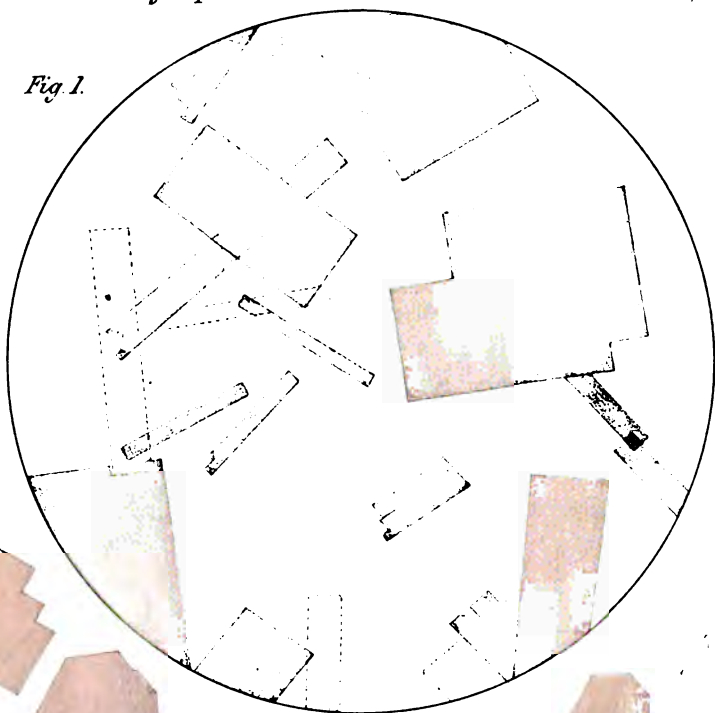


*Fig. 11.*





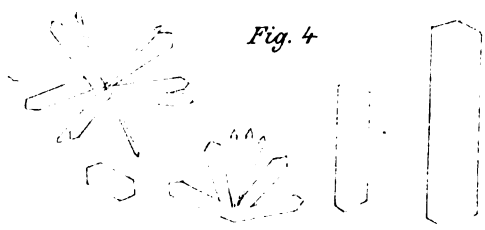
*Fig. 1.*



*Fig. 2.*

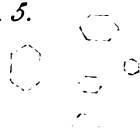


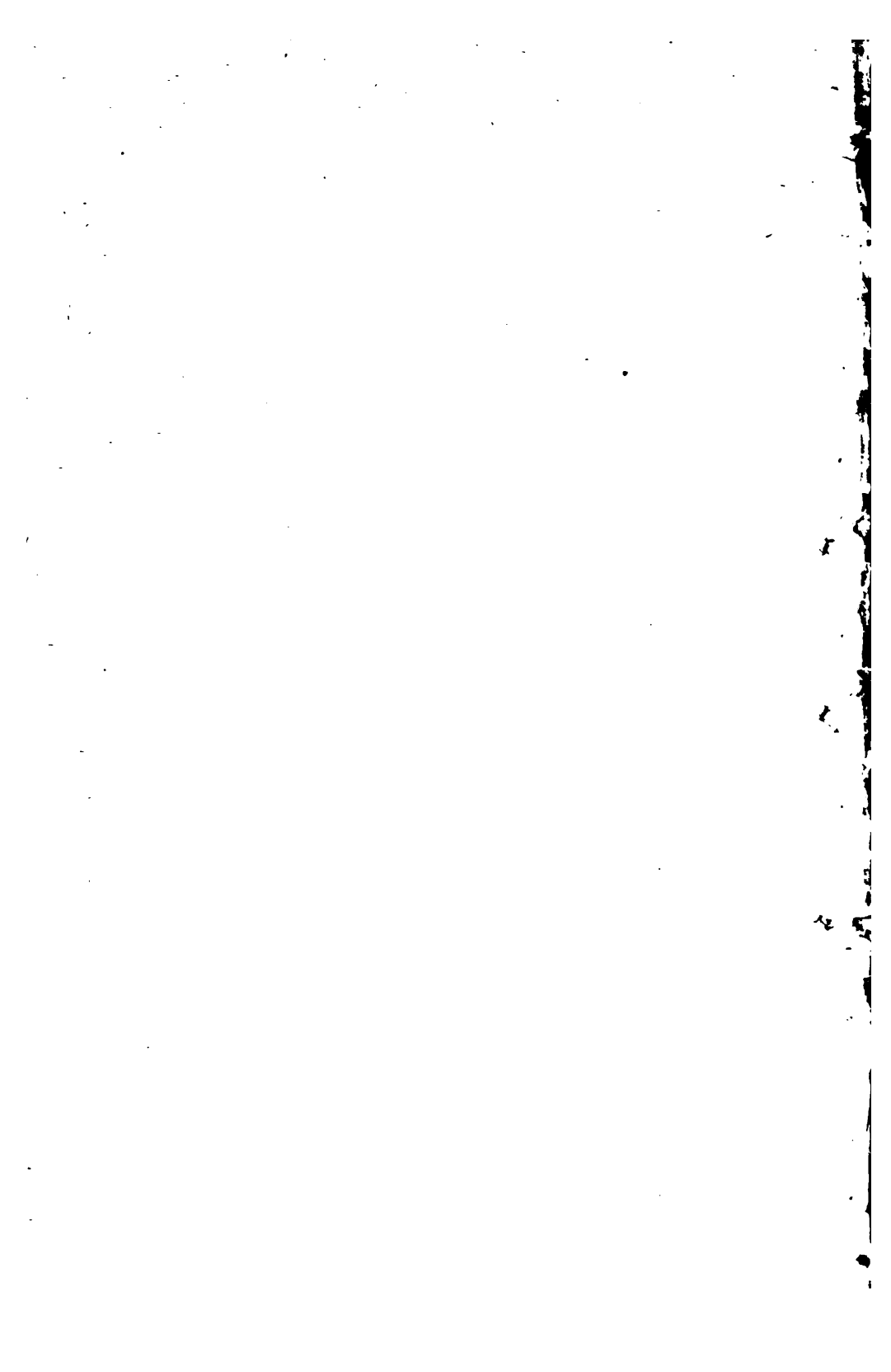
*Fig. 3.*



*Fig. 4.*

*Fig. 5.*





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### OBSERVATIONS IN COMPARATIVE MYOLOGY.

By HANS GADOW, Ph.D., C.M.Z.S.

IN the *Morphologisches Jahrbuch* for 1881, I published two papers on the myology of the reptiles. As the greater part of these papers is descriptive, I intend in the present instance to give the results of my investigations in an abstract and generalised form. This I do under the three following sections :—

In the first section I discuss the important question of the naming of muscles according to a uniform morphological principle.

In the second section, which deals with the differentiation of muscles, I endeavour to illustrate the changes which muscles of the vertebrata may undergo, by adducing some of the most striking examples, and I try to point out the primary causes why, and the conditions under which, these changes take place.

In the third section I discuss the arrangement of the muscles of the hind-limb, and trace their homologies through the Amphibia and Sauropsida to Man. In this section, also, the value of nerve-supply as a guide to existing homologies is illustrated.

#### SECTION 1.—ON THE NAMING OF MUSCLES.

The idea of naming muscles according to a scientific principle is not new. As far back as the time of Cuvier we find anatomists systematically naming muscles from their origin and insertion. Since then, however, this great step forward in the method of descriptive anatomy has not gained ground, but seems rather to have fallen into disuse, its importance not

having been properly appreciated, or perhaps not understood. As it is now, there are only a few anatomists who have followed, or are following, Cuvier in this respect.<sup>1</sup>

To point out the advantages of this method, and to show that terms so made will hold throughout the vertebrate series, is my object in this section of my paper.

In the tarsonomy of anatomical structures, no doubt names indicating their general shape, or their superficial resemblance to well-known objects, suggest themselves most readily. Hence such ludicrous names as *m. gluteus* (from *γλουταίος*), *m. trapezius*, *m. deltoideus*, *pyriformis*, and many others. Such names served very well as long as a name alone was all that was required; thus, a certain muscle was called *m. quadratus femoris* (a name not a whit better than *m. quartus femoris* of Aldrovandi, = *m. duodecimus femoris*, Steno), and it was the student's business to discover and to remember that this name was meant for an ischio-femoral muscle. Such names are wholly unscientific.

A marked improvement on this was the invention of names indicating the function of the muscles. But such physiological names are not sufficient, unless at the same time they convey some topographic information. A name like *m. flexor carpi ulnaris* is perhaps admissible; but, on the other hand, that of "*rotator femoris*" or "*levator scapulae*" tell us just as little about where to look for these muscles, as do the names "*vastus*" or "*biceps femoris*." Again, such a name as "*sartorius*" or "*custos virginum*" (if we take its representative in the female) conveys little scientific knowledge to our minds.

The reason which led mediæval anatomists to invent such

<sup>1</sup> Of those anatomists who have endeavoured to introduce names taken from the origin and insertion of muscles, must be mentioned, above all, Max Furbinger in the following paper, "*Die Knochen und Muskeln der Extremitäten der Saurier*," Leipzig, 1870; "*Zur vergleichenden Anatomie der Schultermuskeln*," in *Jenaische Zeitschrift*, Bd. vii. 1873 (Amphibia); *ibid.*, Bd. viii. (Reptiles), *Morpholog. Jahrbuch*. The muscles of the shoulder girdle of birds, it is hoped, will come out in the *Morphol. Jahrbuch* of this year. In Holland, De Man, *Vergelijkende myologische en neurologische studien over Amphibien en vogels*, Leiden, 1878. In France, Dugès, in his *Recherches sur l'ostéologie et la myologie des Batraciens à leurs différens ages*, 1836; and in this country, G. M. Humphry, in his *Observations in Myology*, Cambridge, 1872, and numerous researches on myology communicated in various numbers of this *Journal*, have endeavoured to elucidate the study of the muscles by well-chosen morphological names.



names and terms as crura, nates, and testes of the human brain, &c., was certainly not a scientific one. This will be seen from the following argument used by Vesalius:—"Optima nomina obscura sunt eligenda ut discipulis corporis humani partes magis jucundiores reddantur et facilius reminiscantur."

However, this was a question of taste, and it did not matter much what a muscle was called so long as the name was merely a mark randomly assigned to it. But when once we study anatomy comparatively we see the danger of physiological names, since we find that the same organ fulfils different functions in different animals, and that the function of an organ itself is the first to vary, not the organ. If we trace a certain organ throughout a large group of vertebrata, we often find that its functions in one animal may have become completely different from, and in the case of muscles even antagonistic to, the function of the same organ in another animal. In every animal of the group a complete morphological homologue of the organ in question may be present notwithstanding a total dissimilarity in function; but it is morphological not physiological homologies which comparative anatomy demands. "Physiological homologies" are analogies, and these, although interesting, are worthless for the purpose of the anatomist. This is shown by Ruedinger, in his essay on "Die Muskeln der vorderen Extremitäten der Reptilien und Vögel." In this essay, elaborate as it is, he arrived only at analogies, since he took the functions of the muscles as his chief guide for comparison, and thus either disregarded the most obvious relationships or classed together the most heterogeneous muscles in different animals.

We see then that, for comparative studies, names cannot be allowed which are taken either from the shape or function of muscles. Comparative or scientific myology requires morphological names, and these can only be derived from origin and insertion of the muscles,—certainly not from the nerves, because these have to be named from the muscles they supply.

The advantages of such a nomenclature are clear, after it is once understood, *that of a compound muscle-name the first part is always to be used in noun-form, and that that is taken from its origin; the latter with an adjectival ending, from its insertion.* If the muscle connects three or more different bones, we have to

frame a name which will contain all these three or more different parts. Thus we are able to understand what is meant by a *m. ischio-femoralis*, since that must be a muscle which arises from the ischium, and is inserted into the femur. Any one, who knows the human skeleton, will know further that, if we speak of a *m. caudi-femoralis*, this must be the *m. pyriformis* of human anatomy. Again, a *m. pubi-ischio-tibialis* will arise from the ossa pubis et ischio, and be inserted into the tibia. A muscle arising from the ischium and from the femur, and inserted into the tibia, would be a *m. ischio-femoro-tibialis*; but if arising only from the ischium, and being inserted into femur and tibia, we would call it *m. ischio-femorali-tibialis*. If there happen to be several muscles connecting the same bony elements, we can easily distinguish each by adding some restrictive adjective, which should, if possible, explain at the same time the position of the muscle with reference to the main axis. Such terms are *ventralis e dorsalis*, *externus e internus*, *sublimis*, *superficialis e profundis*, *anterior e posterior*, *medialis e lateralis*, and even *proximalis e distalis*, or *caudalis e truncalis*.<sup>1</sup>

Thus two muscles arising from the ischium, and one being inserted into the neck of the tibia, while the other is attached lower down, may well be distinguished as *m. ischio-tibialis proximalis* and *distalis*. Terms referring to size and shape, like *major*, *minor*, *longus*, and *brevis*, ought to be avoided.

As to the question which end of a muscle is to be regarded as its origin and which its insertion, we must bear in mind that all muscles are originally trunk-muscles; therefore, the origin of a muscle is the proximal, the insertion the distal part, no matter which is the punctum fixum. Hence it is wrong to speak of a femoro-caudal muscle. This rule is inapplicable to the head.

In the case of muscles which connect serially homologous elements, uniformity in nomenclature would be gained by regarding the more anteriorly situated element as the origin, that which is nearest to the head; the terms distal and proximal have then to be replaced by caudal and capital or cephalic, thus *thoraco-lumbalis* (if there were such a muscle) is preferable to *lumbo-thoracalis*.

<sup>1</sup> I use *truncalis* in preference to *axialis*, as most vertebrates have a distinct tail.

Now, as to the practicability of such a nomenclature. If we only describe one individual muscle, there can be no doubt that we always can find a very good morphological name for it,—a name which indicates its origin and insertion as well as its position. The possibility of a morphological name in individual cases is certain; but in comparative myology we meet with a great difficulty,—a difficulty which undoubtedly is the reason why such a nomenclature has not been universally adopted.

Names given to the muscles of a crocodile may not hold good for those of a lizard, although each muscle of the crocodile may have its homologue in the lizard. Of course, we must allow a certain amount of difference between two homologous parts; for if we press that term too much, there would hardly be a single clear homologue in two different groups of animals. Thus, strictly speaking, the cervical part of the vertebral column of man is not homologous with the neck of the three-toed sloth. In order to obviate this difficulty, Prof. Gegenbaur has invented the term "incomplete homology."

As we shall see later on (page 507), muscles frequently undergo changes by altering their origin, and there seems to be a tendency in limb muscles to shorten, by exchanging their primary origin for a more distal one. We can, for instance, imagine that in a reptile the principal extensor cruris, the dorsal muscular mass of the thigh, arises from the ilium, and forms the continuation of the abdominal muscles, and is inserted into the head of the tibia. Such a muscle we would call a *m. abdomino-ilio-tibialis*. But in another animal the muscle may have given up its connection with the abdominal wall, in which case it would be a *m. ilio-tibialis*. But surely no one would ever object to call this ilio-tibial muscle not only the representative, but even the homologue of the former.

There are, however, cases in which names given to the muscles of amphibia do not suit those of mammals or reptiles. Again, there are muscles which vary so much, even in one group of animals, that it is quite impossible to find satisfactory names for them. This difficulty applies equally to morphological and to physiological names. Examples of such muscles are, in the hinder extremity, the *m. ambiens*, the *flexor tibialis* mass, and the muscles of the leg and of the toes.

The *ambiens* muscle, for instance, if typically developed (as in the crocodile), is a muscle which arises from the pelvis (either from the ilium or from the pubis), and is inserted into the plantar side of the toes, having a distinct tendon as it passes over the knee. In some other reptiles the muscle is completely broken up into two at the knee, its proximal half being inserted into the tibia directly or through the tendon of the *femoro-tibialis*, and its distal half having become a *m. peroneus posterior*. Therefore, in the crocodiles, the proximal half of the *ambiens* muscle acts as a flexor of the toes and as a rotator of the leg; while in other reptiles the part homologous with the proximal half of the *ambiens* is a levator or an extensor of the leg, and has no action upon the toes at all. We cannot, therefore, find either a morphological or a physiological name which is applicable to the various conditions of this muscle in all the different reptiles.

Another illustration of the difficulty of giving suitable names to muscles is afforded by the mass generally called *mm. semitendinosus* and *semimembranosus*, which I have described as *mm. flexores tibiales internus et externus*. They always act as flexors of the leg, but their origin varies much, since they arise either from the ischium alone, or from the ischium and ilium, or, as in *Hatteria*, from the anterior caudal vertebræ.

As regards the muscles of the leg and toes, Professors Fürbringer and Hoffmann have attempted to invent names according to the morphological principle, but in doing so they met with two difficulties, the consideration of which shows us that consistency must not be pressed too far.

The first difficulty is, so to speak, a morphological one. The *m. gastrocnemius*, for instance, may arise either from the femur and the tibia, or from the condyles of the femur only; or the *m. peroneus posterior* in crocodiles is the continuation of the tendon of the *ambiens* muscle (as stated before), but also, at the same time, that of the *m. extensor tibialis* and of the *m. femoro-tibialis*; while in the lizards it arises from the external condyle of the femur, and in chameleon from the fibula. The insertion of these muscles is still more variable. It would, therefore, in many cases be impossible to derive from their origin and insertion a name which would be applicable to more than a few animals at one time. The second difficulty we may

express as an etymological or better practical one. Names like "*m. epicondylo-fibulo-tibio-digitalis ventralis profundus*" (Fürbringer), and "*m. femoro-tibiali tarsum-metatarsum v.*" (Hoffmann), are no doubt too clumsy, and if we were to follow the principle indicated by them, we should in the different vertebrata be led into every imaginable combination of *femoro-*, *tibio-*, *fibulo-*, on the one side, and of "*tarsalis-*, *metatarsalis-*, *digitalis i.-v.*" on the other.

The systematic application of such enormous names is impossible, for they are little better than a cumbersome description of the muscle, to avoid which is the purpose of a name. For the muscles of the legs and toes it seems therefore impossible to give strictly accurate names, and here our principle would seem rather to fail. But fortunately most of the names of the muscles of that region are tolerably good, since most of them, such as *mm. tibialis anticus*, and *peroneus*, are at least somewhat topographical, and thus infinitely better than *mm. semitendinosus*, *scalenus*, *teres*, and other mediæval inventions. After all, "what can't be cured must be endured." However, this homely proverb does not apply to the vast majority of muscles, as we find, if we take a broad view of the muscles of the vertebrata on the whole, but only to a few exceptional instances.<sup>1</sup>

It was one of my chief objects when preparing my paper on the myology of reptiles to use only such names of muscles as might

<sup>1</sup> We cannot derive a name from the study of *one* group of animals alone. This leads to mistakes, and to the coining of names perhaps very attractive, but which have almost invariably to be suppressed for less detailed names, in order to make them bear generalisation. Professor Hoffmann, for instance, in Bronn's *Classen u. Ordnungen des Thierreichs*, invented names for the muscles of the chelonians, which are suitable for them alone, but utterly valueless for other vertebrata; this criticism does not apply to the muscles of most animals, however, as the majority of them have been worked out in several groups, and named, in accordance with the general principle here advocated, by previous anatomists, of whose figures and descriptions he has, as is right, made the fullest use. I myself am responsible for the introduction of several bad names in my paper on the myology of the hinder extremity of the Ratitæ. Names such as *rectus femoris internus*, and *caudi-ischio-ilio-flexorius*, might have been easily made applicable to other vertebrata, as well as to birds, if at the time I had only had a better acquaintance with the myology of the reptiles. From what will be said in the next section, it will be seen that the names, which are taken from the study of lower forms, are usually preferable, for they have a greater chance of being more widely applied. Hence most of the names invented by De Man for the muscles of *Salamandra* may be well used for most of the higher vertebrata.

bear generalisation, or, in other words, as might be also applicable to amphibia, birds, and mammals. That this is possible was there demonstrated, except in the two cases to which I have just referred. But because I did not succeed in finding proper names for these muscles does certainly not prove that somebody else might not do so. For instance, a muscular mass, which, like the *m. flexor tibialis*, is inserted into the proximal end of the leg (tibia or fibula), and arises either from the pubis or ischium, or from both, or from the angle between the tail and ischium, might perhaps with advantage be called *pelvo-cruralis*, with some restrictive adjective as *superficialis*, *internus*, *ventralis*, or *posterior*; or it might be called *pelvo-perineo-cruralis*. The difficulty is that we have at present no term for the region between the os ischii and the tail.

## SECTION 2.—ON THE DIFFERENTIATION OF MUSCLES.

If we examine the muscles of a lizard and those of a salamander, we meet with many instances in which at first sight it seems impossible to point out the homologies of the separate muscles. However, if we study a larger series of forms, we find almost every possible variation in the shape and in the grade of development of their muscles, which form the intermediate stages, and so enable us either to derive such muscles one from another, or to trace them back to one mass originally common to the ancestors of both. Thus we can perceive how a muscle not present in one animal is developed in another. This kind of *neomorphism*, the development of a given muscular mass or stratum into several special new muscles, can be effected in six different ways.

*1st Case.*—*The original muscle may break up into a proximal and into a distal segment.*—The result of such a division is that two muscles are found, a proximal and a distal one.

As a rule, this process begins by the formation of a tendinous intersection. Examples of this are afforded by most of the long and short flexors of the toes, the *m. ilio-tibialis* on the dorsal or anterior aspect of the thigh, and the *m. pubi-ischio-tibialis* on the ventral or inner aspect.

The proximal end of the *m. ilio-tibialis* originally formed a more or less continuous stratum with the neighbouring parts of

the *m. obliquus externus abdominis*. This condition is still preserved in Hatteria, and in the Urodela in a still higher degree, —as, for instance, in Menobranchus and Menopoma (Humphry and Mivart), since in these animals the *m. ilio-tibialis* scarcely takes its origin from the iliac bone, but forms simply the continuation of a part of the *m. obliq. externus*. Again, in Ceratodus the oblique abdominal muscle is one continuous mass with the dorso-lateral musculature of nearly the whole limb.

The condition as shown in Ceratodus may be considered as the primitive stage. The great development of the pelvic girdle had, *inter alia*, the result that, for merely mechanical reasons, the dorso-lateral muscular mass of the limb became partly attached to the iliac bone. This, as in many other cases where a muscle passes over a bone, caused the formation of a tendinous intersection, which process again led to the entire division of the whole muscular mass into a proximal part, the *obliquus*, which thus became attached to the iliac mass, and into a distal part, the *m. ilio-tibialis*.

The reptiles alone show us every intermediate stage. This differentiation of the muscle into a proximal and a distal segment was of great advantage to the reptiles, since the two muscles became capable of acting independently of one another, and the limb could execute more specialised motions. We must also remember that the change of an aquatic animal, which used its limbs simply like paddles, into a semi-aquatic and partly terrestrial, and therefore crawling, creeping and running creature, involved a complete change of its muscles, and at a later period of its bony framework. In swimming animals, such as Ceratodus and Menobranchus, the longitudinal axis of the whole limb is nearly straight, whilst a terrestrial life necessitates the bending of the limb at several angles and the development of more or less complicated joints.

In a similar way as the *m. ilio-tibialis* does on the dorso-lateral aspect of the thigh, so the mass called *m. pubi-ischio-femoralis* forms the continuation of that part of the *m. obliquus externus* which is situated mesiad from the thigh. The tendinous intersection is still represented by the *ligam. pubi-ischiadicum*, extending from the proc. lat. pubis to the symphysis ischii. In Hatteria, in some other Saurians, and also in some of the lower

Amphibia, particularly, *e.g.*, *Cryptobranchus*, the external oblique abdominal muscle forms *one* continuous layer with the proximal part of the *m. pubi-isthio-tibialis*, a tendinous intersection, or *lig. pubi-ischiadicum* being not yet developed.

All the long flexors and tensors of the toes are only the distal parts of muscles, which, originally arising from the trunk or pelvic girdle, extended down to the toes as *one* continuous and very simple muscle. The latter conditions still obtain in *Cryptobranchus* (Humphry in this *Journal*, vol. vi.).

The development of a knee-joint proper, and the bending of the thigh to the leg at an angle of about  $90^{\circ}$ , must have caused the long extensor muscles passing over the knee to develop a tendon, which again led to their partition. In the reptiles the hind-limbs have reached such a point in development that intermediate or primitive stages have become scarce. But there are still some cases where we find muscles which show us how the breaking up into a proximal and a distal part, *i.e.*, into thigh muscles and toe muscles proper, took place. There is, firstly, the *m. ambiens*, the tendon of which in the crocodiles and in many birds passes right over the knee, and forms one of the heads of the long flexors. In other reptiles the tendon of this muscle is inserted on the knee, and the heads of the flexors of the toes arise only from the tibia and fibula, and that part of the tendon connecting the *ambiens* with the muscles of the toes has disappeared.

The whole process can be explained in the following way:— In the 1st stage, one flat muscle arose from the pelvis, passed along the whole limb on its antero-medial aspect, and was inserted into the toes; 2nd stage, in order to avoid injury by pressure on the muscle, it lost its muscular elements in the neighbourhood of the knee and developed connective tendinous tissue; 3rd stage, the distal half of this long muscle increased the number of its muscular fibres, and as some of these attached themselves to the underlying bones (fibula and tibia), this portion of the muscle became enabled to act upon the toes by the contraction of its fibres alone, as it now had a *punctum fixum* independent of the proximal part. When the two parts contracted, the action upon the toes was of course stronger, and the *whole* limb was rotated round its longitudinal axis; but only if



slight action upon the toes was required, it was solely the fibular head of the muscle that came into play.

If the animal did not require the complicated rotating action which resulted from contraction of the whole muscle, the distal part became more strongly developed, and the tendon between it and the proximal part was no longer pulled upon. Being thus put out of action, the proximal part would have become aborted, and perhaps would have gradually disappeared (in many birds and in mammals the *ambiens* muscle has actually disappeared) had its existence not been saved by the tendon becoming attached to the knee itself, or to the distal tendon of the *m. femoro-tibialis*. By degrees the connection of the proximal and distal parts was severed. The latter now forms a separate muscle—the *flexor digitorum*.

The advantage of this arrangement is obvious, since the animal now is enabled with the same amount of muscular material to perform two very specialised actions, namely, extension and elevation of the leg; and secondly, flexion of the toes, without one movement interfering with the other.

We must not forget, however, that the separation of the proximal part from the distal might have begun in a manner quite different from that which we have just indicated. Supposing the tendon of the whole pelvo-pedal muscle, whilst passing over the knee first became connected with it, the broad distal end of the femoro-tibial or of the ilio-tibial muscle might, for instance, first overgrow, then surround, and lastly attach some of its fibres to some of those of the *ambiens* tendon (in many birds and in the crocodiles, the tendon of the *ambiens* is actually surrounded by the other muscles in the region of the knee), or the *ambiens* tendon itself might become partly attached to neighbouring parts. The result of such an arrangement would be that thigh and leg would gain a new extensor and levator, viz., the proximal part of the pelvo-pedal muscle, while the distal part would be compelled to degenerate into a useless tendon, if its muscular belly did not become attached to the fibula (*e.g.*, in Chameleon), or some of the strong inserting tendons of the adjacent muscles.

The breaking up of what were originally pelvo-pedal muscles is also illustrated by the long tendon on the hind-aspect of the leg, which descends from the *m. flexor tibialis externus* to the foot; this

arrangement is typical of the Urodela, and is frequently found in reptiles, in birds, and even in some mammals.

Another example is to be found in some of the long flexors of the toes, since these muscles indefinitely arise from the condyles of the femur alone, or from the femur and the tibial fibula, or only from the latter bones.

The tarsal region affords perhaps the best proof of the fact that muscles when they pass over a joint, and so consequently rest upon a ridge, or when they are bent at an angle, can easily break up metamerically into several new muscles. Some of the long flexors pass the tarsal joint without undergoing any change, while others become partly attached to the underlying bones, or the part which originally passed over the joint may become completely inserted into the calcaneum or into the astragalus, and the remainder, the distal part, becomes transformed into a short flexor of the digits. The reptiles exhibit every imaginable intermediate stage, but we may point out that the arising of the short flexors from tendons of the long flexors is an exception in mammals, while in the Amphibia it is the rule.

*2nd Case.—Division of a muscular mass into layers.*—This way of increasing the number of muscles has led in the pelvo-femoral region to the development of a proximal or deeper, and a distal or superficial cone of muscles. Those muscles formed by the splitting of a muscle-mass into several layers, as a rule are supplied by the same principal group of nerves; while, on the other hand, muscles formed by longitudinal division, as described in the 3rd Case, a process which decreases the number as we ascend in the scale of animal life, receive their nerves from different groups (see the tabular arrangement given on page 510).

Examples of the differentiation of a single muscle into new ones by division into layers are the abdominal muscles, which are developed from the primary lateral muscular mass of the body wall (see *Morpholog. Jahrbuch*, 1881, p. 57).

Again, the *m. ilio-femoralis*, the *m. femoro-tibialis*, and the *m. ischio-femoralis* are genetically connected with one another, and to judge from the conditions shown in the Selachians, in *Ceratodus*, *Menobranchus*, and *Cryptobranchus*, they originally formed one large mass, which mass, in continuation with the oblique abdominal muscles, arose from the trunk, and

extended its insertion from the femur all over the antero-lateral aspect of the leg right down to the toes. Such a muscle acted by its contraction as a levator of the whole limb, until the development of a proper knee-joint involved at the same time a complete change in it, when all the fibres running from the trunk or from the pelvis to the femur became useless or superfluous for the extension of the leg. Again, with respect to the abduction, and in particular to the rotation of the thigh, all the fibres inserted, distad from the condyles, were rendered quite unnecessary. Thus, as long as the whole mass was united, complete mobility of the limb was not only not obtained, but even interfered with, and the motion of its segments in detail was well-nigh impossible. Consequently there existed a certain layer, or part of the whole mass, which was never used at all; this part therefore aborted, while the proximal part concentrated itself into a dorso-proximal deep muscle, which was then used only as a levator and rotator of the femur—the *m. ilio-femoralis*. All the other superficial and distal elements, took the form of a *m. extensor ilio-tibialis*. This muscle again underwent further differentiation, so as to facilitate finer and more specialised motions of the leg; for instance, by extending its origin gradually over the medio-ventral side of the limb towards the pubic bone, and dividing itself longitudinally into an outer and into an inner mass, it finally produced the *m. rectus internus femoris* of the Amphibia. Such a muscle was of great advantage for the animal, as it not only extended the leg, but became also a slight adductor of the whole limb.

With the final development of the adductors and abductors (*mm. ilio-fibularis, pubi-ischio-tibialis, pubi-tibialis, and ambiens*) the head of the ilio-tibial muscle began to give up its origin from the ilium and to take a more distal one from the femur. In the Amphibia its iliac origin is still very fleshy, and forms the direct continuation of the oblique abdominal muscle, while a muscle arising from the femur and inserted into the tibia is not yet developed. The Amphibia, therefore, represent the lowest stage; but in many Lizards and in the Crocodiles the muscular elements near the ilium are much reduced, and the ilio-tibial muscle arises from the ilium only by the help of tendons. Its deeper fibres, however, in course of time, took

their origin from the distal two-thirds of the femur, and thus produced a very strong and serviceable muscle, the *m. femoro-tibialis*, which could fulfil the function now required of it (viz., extension of the leg exclusively), in the most direct way with the smallest possible amount of muscular material.

In birds the most superficial layer of the mass is very much thinned out, and reaches the tibia, not independently, but only by attaching itself to the tendon of the underlying femoro-tibial muscle—the muscle which became so strongly developed at the expense of the superficial parts.

Lastly, in man and in other mammals the suppression of the reptilian *m. extensor tibialis* is carried out to the extreme, as there is nothing left of it, except, so to speak, the mere tendinous or aponeurotic sheath; it has degenerated into the fascia lata or tensor vaginæ femoris.

*3rd Case.—Longitudinal division of muscles.*—This, as a rule, has taken place in such cases where, e.g., in an animal A, a certain muscle is supplied by nerves from the crural and from the ischiadic plexus; while in another animal B, that muscle is represented by two, one of which is supplied by a crural, the other by an ischiadic nerve. Examples of such a longitudinal division are the *mm. pubi-tibialis*, *ambiens*, and *rectus femoris internus*, these having been separated off from the original *ilio-tibialis* mass. Further, the *mm. flexores tibiales* or the adductors proper; these by longitudinal division develop an external and an internal mass, which again are liable to be split up into secondary and very variable parts, even in one and the same species of animal.

Most of the short toe muscles seem to have resulted from the splitting up of an originally broad and flat muscular layer. This view is supported by the fact that, in many cases, the division does not yet extend to the proximal part of the layer.

The muscular mass called *ilio-femoralis* in Urodela and Reptilia, has in birds become differentiated into four muscles (see table on p. 512); this is due to longitudinal division as well as to separation into layers.

The *m. ilio-fibularis* most probably is nothing other than the posterior or most caudally situated part of a dorsal muscular mass, which arose from the whole crest of the ilium, and was inserted into

the tibia and fibula. Cryptobranchus and Menobranchus prove that such a broad dorsal muscle, extending from the ilium to the proximal end of the leg, could quite well exist as long as the limb was rarely, or at any rate not constantly, bent at the knee; but through such a bending the whole dorsal muscular mass became at once subject to the mechanical influence of the modified direction of forces: the whole stratum split longitudinally into two parts,—the one an anterior (*m. extensor ilio-tibialis*), the other a more caudal or dorsally situated one (*m. ilio-fibularis*). The latter, after the bent knee had become the more usual position, could no longer act upon the leg either as an abductor or as an extensor, but became a flexor pure and simple, a function up to that time totally strange to it.

*4th Case.*—*Formation of a new muscle by fusion of two muscles originally separated*, as indicated by their nerve-supply.—This process of course decreases the number of muscles. In reptiles and mammals some of the short dorsal and plantar toe muscles form new lateral muscles by fusion. Instances of this are rare, but that it does take place may be seen in the *m. gluteus posterior*, *m. tensor fasciæ latæ* of the Ratitæ, and many other birds, where these muscles have now become morphologically and physiologically one.

*5th Case.*—*Changing of a muscle in shape and situation by alteration of its origin and insertion.*—In this case the muscular elements are often greatly increased, but may also be decreased in number, and the whole muscle may become so completely altered in appearance that it is impossible to recognise it without the aid of its nerve-supply. As a rule, the origin is more subject to variation than the insertion. The *m. quadratus lumborum* shows how extensively the inserting part of a muscle can change its place. The *ambiens* muscle varies also very much in its mode of origin. In Alligators it arises from the anterior spine of the ilium, but in Hatteria and in Testudo from the *processus lateralis pubis*; in Monitor, Lacerta, and others from the inner and ventral aspect of the preacetabular part of the ilium, close to the acetabulum; while in other Saurians, as in Iguana, Pterodactylus, and Chameleon, its origin has passed over to the acetabular part of the os pubis. The distal end of the *ambiens* muscle may either pass over the knee and form the head of the flexors of the toes

(Crocodiles), or its tendon may become attached to that of the ilio-tibial and to that of the femoro-tibial muscle (Hatteria and most other reptiles).

The most striking example of the extent of change which may be undergone by a muscular layer is afforded by the *m. pubi-ischio-tibialis*. We can imagine seven different conditions or forms under which this muscular mass may be developed.

*1st Form.*—The superficial muscular layer on the inner side of the thigh (originally the continuation of the *m. obliquus abdominis*) arises not only from the whole of the *ligam.-pubi-ischiadicum*, but also from the *symphysis ischii*, and eventually from the *os cloacale*. It is inserted broadly into the inner aspect of the proximal part of the tibia. The part between that bone and the *os pubis* is supplied by nerve branches from the crural plexus, or from the obturator nerve, while the rest of the muscle, the more posterior part, receives branches from the *plexus ischiadicus*. Thus we have a muscle which in origin and nerve-supply is thoroughly *pubi-ischiadic* (Hatteria).

*2nd Form.*—In order to use this broad muscle as a mere adductor, and on the other hand as an adductor-levator of the leg, the muscle splits longitudinally into two, and we have then an ischio-tibial and a pubi-tibial muscle, each with a different source of nerve-supply. This form is found sometimes in Monitor.

*3rd Form.*—If the function of the pubic or anterior part of the muscle becomes superfluous, or is no longer required, it becomes aborted, and leaves as its representative only the posterior part; this, of course, is only incompletely homologous with the whole primitive mass, and from its nerve-supply and origin we might now call it a *m. ischio-tibialis*.

*4th Form.*—Or the posterior part may disappear, and the crural remain; we then have a pubi-tibial muscle (in mammals known as *m. gracilis*).

*5th Form.*—The whole muscle may become unnecessary, and may disappear altogether (Crocodiles).

Lastly, we have to mention complications of the above stages. The conditions, in particular the extent of the origin and the mode of nerve-supply of this muscle in certain Saurians, make the following hypothetical case probable:—

*6th Form.*—The ischio-tibial part, representing the 3rd form, after the crural part has disappeared, can increase the number of its fibres, and by creeping along the *lig. pubi-ischiadicum* can extend its origin up to the *proc. lat. pubis*. Such a muscle, if it were not for its exclusively ischiadic nerve-supply, would have a striking resemblance to the original pubi-ischio-tibial mass.

*7th Form.*—The crural part might greatly multiply its elements, when we should have a muscle arising from the whole pubi-ischiadic ligament, and inserted into the tibia, but with crural nerve-supply.

*6th Case.*—Any muscle may become superfluous, either because it may be put out of action by a given position of the limbs becoming permanent, or because the work hitherto done by the muscle can be better done by neighbouring muscles, or because its special activity is not required any longer. In all these cases the muscle will become aborted, and will either form an accessory supporting part to another muscle, or it may become converted into an aponeurosis and finally disappear, without leaving any trace of its former existence.

### SECTION 3.—ON THE ARRANGEMENT OF THE MUSCLES OF THE HIND-LIMB.

TABLE I.—*The Muscles Arranged according to their Insertion.*

I. *Inserted into the pelvis—*

M. oblig. abd. ext.  
 quadrat. limb.  
 rect. abdomin.  
 ilio-caudal.  
 ischio-caudal.

II. *Inserted into the thigh—*

M. quadr. lumb.	} dorsal mass.	} proximal or deep cone.
ilio-fem.		
caud. il. fem.		
caudi. fem.		
pub. isch. fem.	} ventral mass.	
ischio-fem.		

III. *Inserted into the leg—*

M. pubi-tibial.	} dorsal mass.	} distal or superficial cone.
ambiena.		
femoro-tibial.		
ilio-tibial.		
ilio-fibul.	} ventral mass.	
MM. flexores tibiales		
M. pub. isch.-tib.		

TABLE II.—*The Muscles Arranged according to their Innervation.*

Plex. cruralis + n. obturat. exclusively.	Plex. ischiad. + post. sacral nerves, exclusively.	Plex. ischiad. + n. obturat. (ventral muscles).	Plex. crural + n. obtur. (dorsal muscles).
Mm. obliqui abdomin. M. quadr. lumb. pubi-tibialis. ambiens femoro-tibialis. pub. isch. fem. int.	lateralis caudæ. candi-femoral. cand. il. fem. ilio-fibular. flex. tibial. ext. pub. isch. fem. posterior.	pub. ischio-tib. ischio-femoral. flex. tibial. int. pub. isch. fem. ext.	ilio-femoralls. ilio-tibialis.

We learn from these tables that the muscles of the hind-limb of the reptiles cannot be arranged in natural groups, either by their insertion alone, or by their relation to the two principal nerve plexuses of the pelvic region. Their origin affords a still less satisfactory guide, in consequence of the great variations to which it is subject.

An arrangement founded upon the nerve-supply of these muscles would likewise be impossible, since many of them are supplied by both plexuses, and the lower the stage in which the animals stand in their general development the greater is the number of such muscles with a double nerve-supply.

Again, the nerve-supply cannot be the principal character in cases where the same muscle as the Saurians has in one species a crural, and in the other an ischiadic nerve supply. This may be illustrated by the ischio-femoral muscle of Saurians. This muscle in *Ophryæssa* and *Monitor* is supplied by branches from the obturator-nerve and from the ischiadic plexus, while in *Iguana* and *Lacerta* it receives them only from the latter; again, in *Crocodiles* and *Chelonians* it is supplied only by the obturator nerve. Now, supposing we did not know the conditions of this muscle in such forms as *Ophryæssa* and *Monitor*, we should feel perfectly justified in saying—if we take the nerve-supply as our principal guide—that the ischio-femoral muscle of a *Crocodile* is totally different from that of *Iguana*, in spite of all similarity in situation, origin, and insertion. And yet, as shown by *Monitor*, it is one and the same muscle, it is, however, not completely homologous in the different animals, in so far as, in *Crocodiles*, it has lost or perhaps has not yet developed the ischiadic part, while in *Iguana* the crural part is absent.

Another illustration of such incompletely homologous muscles



is shown by the many changes undergone by the pubi-ischio-tibial muscle.

The cases just narrated show that nerve-supply cannot well be used for separating the muscles into groups, but prove beyond doubt that the innervation is of the greatest value, and often the only guide we have by which to work out the genetic relations of muscles. It follows, therefore, that any classification of the numerous muscles of the hind-limb will have to be based upon their mutual relationship. To ascertain this, we must revert to earlier but typical forms where the muscles are fewer in number; and, lastly, we must trace even these back to their earliest primitive conditions. This might be done perhaps ontogenetically by embryological research. In our case, however, we proceed phylogenetically.

On comparing the muscles of the reptiles with those of man, it is seen what phylogenetically has actually become of the muscles of the reptiles; whilst, on the other hand, if we compare them with those of the much lower Urodela, we see from what they have been developed. In other words, we see both past and future of such muscles. The following table is an attempt to arrange the muscles of the posterior half of the vertebrate body into groups, and to point out into what muscles the primary muscular mass of the lateral body wall has been differentiated in the Urodela, Reptiles, Birds, and in Man. The Anura are not included in this table, since these animals are specialised into forms, which are so aberrant that they would only mislead us in our attempts to understand the higher vertebrata. The musculatur of the frogs is in many respects more highly differentiated than that of man.

TABLE IIIa.

Muscular Stratum of the Body Walls.				Urodela.	Reptiles.	Birds.	Man.
	Outer.	Middle.	Inner.				
A.—PRESACRAL OR TRUNCAL REGION PROPER.	+				M. obliquus abdominis externus		
		+		Intercostal mass	Serrati Intercostal ext. Quadrat. lumb. Intercost. int. Scalares	Intercost. ext. et longi ? Intercostales interni	Serrati + scaleni Quad. lumbor. + ilio-psoas
			+		Obliquus abdominis internus + Transversus abdominis		
B.—SACRAL OR PELVIC REGION. Lateral or Dorsal.			+	<i>Ilio-femoralis</i>		Iliac. ext. med. et anterior <i>Iliac. ext. post.</i> Glutæus ant.	Glut. med. et minimus (pt.)
	+			<i>Ilio-tibialis</i>	<i>Ilio-tibialis</i> (in Saurians) + ambiens and pubi-tib.	Extens. ilio-tib. + ten. fasc. lat. Ambiens Sartorius <i>Glutæus post.</i> femoro-tibialis + rect. fem. int.	Tensor vaginæ femoris Rect. int. fem. (pt.) Sartorius Mm. vasti + crureus
		+			Ilio-fibularis		
Medial or Ventral.			+	Pubi-ischio- femoralis internus	Pub.-isch.-fem. int. pars I. + II. pars III.	Iliacus int.	Biceps, + Glut. maximus (pt.) Pectineus Ilio-psoas ?
				<i>Pubi-ischio- femoralis externus</i>	<i>Pub.-isch.- fem. extern.</i> Pub. isch. fem. posterior	Pub. isch. fem. (pt.) Obturator	Obtur. extern. Gemelli Quadrat. fem. Obtur. inter.
		+		<i>Ischio-femoralis</i>		Pub. isch. fem. (pt.) + ischio-fem.	Adduct. longus brevis <i>magnus</i>
Caudal.	+			Pub.-isch.-tib. + pubi-tibialis	<i>Pub.-isch.-tib.</i> (only in Lizards)		Gracilis
		+	+	caudal-femor.	caudi-femor. caudi-ilio-fem.	caud. isch. il. fem.	Pyramiformis
	+			caudal-pubi- ischio-tib.	Flex. tib. ext.	caud. il. flex.	Glut. max. (pt. + Semitendin).
	+			ischio-flexor	<i>flex. tib. int.</i>	ischio-flex.	Seminimbran.

N.B — The muscles in italic have a double nerve-supply.

TABLE IIIb.—*Muscles of the Tail.*

	Urodela.	Crocodil.	Saurii.	Chelonii.	Aves.
C. TAIL OR PERTURSEAL.	Dorsal.	M. caudæ dorsalis (continuation of the m. longissimodorsi of the mus. inter-spinales, &c.)		Levator caudæ	
				(testococcygeus)	(levator coccygis)
	Ventral Muscles.	Pars iliaca	ilio-caudalis	ilio-sacro-caudalis	levator coccygis (pt.)
		Pars ischiadica (Superficial layer)	constrictor + retractor cloacæ	Sacro-caudal.	Depressor coccygis
		(middle layer)		Sphincter cloacæ	
		(deeper layer)	ischio-caudal + dilatator cloacæ	ischio-caud. pubi-caud.	pubi-coccyg. externus
			+ mm. genitalium	lumbo-caud.	
				+ retractor penis vel clitoridis	pubi-coccyg. internus
					Mm. genitalium
					m. caudifemoralis
		(deepest layer)	M. caudi-femoral		

The muscles printed in italics are supplied by nerves from two different plexuses.

From this table we can draw the following conclusions:—

(1) The number of pelvic and thigh muscles proper (division B), increases considerably from the Urodela, through reptiles and birds to man, there being developed in each 11, 15, 18, and 21 different muscles respectively.

Again, as indicated by the table, this increased number in the higher vertebrata results from the division of the muscles already present in the next lower group. In other words, the Urodela is less specialised than the reptile, and this again is surpassed by the mammal. It may seem trivial to mention these well-known facts, but it certainly is of interest to observe that the muscular system marches step by step with the whole organism along the lines of its development.

(2) Specialisation of the muscles is likewise indicated by the

proportion in which muscles with double innervation (*b*) stand to the total number of the muscles (*a*). Thus in the Urodela, with a total number of 11, there are three muscles belonging to the *b* category, *i.e.*, 27 per cent.

In the reptiles, taken as a whole, these numbers would be in the proportion of 16 : 6, the percentage being about 37. This would seem to be contrary to what we might expect, but we have to bear in mind that the Crocodiles, Saurians, and Chelonians are widely different from each other in their muscular arrangement. The numbers in the single groups are—

	Crocodiles	Monitor.	Other Lizards.	Chelonia.
<i>a</i> (Total number), . . . . .	10	11	12	11
<i>b</i> (With double nerve-supply), . . . . .	3	4	3	3
Percentage, . . . . .	23	27	25	27

No fair average therefore can be struck for the reptiles. But an indubitable decrease of such doubly-innervated muscles takes place in the Ratitæ; and as these birds possess at least three muscles more than the Saurians, the proportion of *a* : *b* must be considerably less than in either Reptiles or Amphibia. It is about 12 per cent.

Lastly, in *man* it is normally only the *m. adductor magnus* which is supplied by nerves from the ischiadio and from the crural plexuses, and as in *man* about twenty muscles are developed in the pelvo-general region, the percentage would be no more than 5.

(3) As it is chiefly the muscles of the group *b*, by the differentiation of which in the next higher class of animals an increased number is produced, we are justified in supposing that a muscle represents a lower stage if it has a nerve-supply from two different plexuses,

FAT EMBOLISM. By ROBERT SAUNDBY, M.D. Edin., and  
GILBERT BARLING, M.B. Lond., F.R.C.S. Eng. (PLATE XIII.)

ALTHOUGH the subject of fat embolism has been before the profession for twenty years, and has been the subject of many important contributions to periodical literature, it has been slow in obtaining any general degree of acceptance, and is still mentioned exceptionally in our text-books.

In 1862 Zenker noticed that embolism was produced in the lungs by fat introduced into the veins in the case of a man who had been crushed, but the condition was not considered by him as of much importance. In the same year Wagner made a similar discovery, but regarded it as rather the result of a pyæmic condition than as the consequence of the crushing of fat-containing tissue. But in 1865 Wagner and Busch published further independent accounts of fat embolism, showing that it occurs in all cases of fracture of bones to a greater or less extent, that the embolism generally implicates other organs besides the lungs, and that it gives rise to symptoms which are capable of being recognised during life. They suggested that it might explain some cases of death hitherto ascribed to shock. Moreover, they showed that the embolism varied in amount in direct proportion to the extent of injury to the cancellous tissue of the bone, and that the fat passed into the circulation by the great veins and larger lymphatics. Further, they suggested that embolism may occur in cases of bone degeneration without injury. Dr. Busch's paper contained an account of a case in which death had actually resulted from fat embola; yet very little notice seems to have been attracted towards the subject until the publication of Professor Czerny's<sup>1</sup> paper towards the end of 1875. Czerny related the case of a man admitted with a transverse fracture of the thigh. The first abnormal symptom was the rise of temperature to 102°·6 Fahr., which took place on the day after the injury. In the evening his pulse and respirations were rather rapid; and as he was restless and unable to sleep a little morphia was given him at 9 P.M., and as this

<sup>1</sup> *Berlin Klin. Woch.*, 1875, Nos. 44 and 45.

had no effect it was repeated just before midnight. A few hours later his breathing became loud and rattling, and on examination he was found to be quite comatose, cyanosed, and breathing deeply and rapidly. There were loud bubbling râles in the chest, but the percussion note was clear and somewhat tympanic. The pulse was 100, full and strong; the pupils were contracted, and insensible to light. There was no reaction to external stimuli. He died at 7.30 A.M. on the third day, about thirty-eight hours after the injury. The last symptoms observed were convulsive twitchings of the arms, profuse perspiration, and tracheal râles. At the *post-mortem* examination the lungs were found cedematous, and marked with small bright red spots and streaks, and on microscopical examination the smaller arteries and capillaries were distended with fluid fat, which was equally distributed over both lungs. Branching fat embola were found in the brain, in the pia mater, and cerebral substance, their sites being indicated to the naked eye by numerous punctiform ecchymoses. The liver and kidneys also contained a number of similar embolisms.

In 1877 Dr. Arthur Boettcher<sup>1</sup> reported a case of death from fatty embolism of the pulmonary vessels after a gunshot wound of the knee-joint. There was in this case an unusual abundance of subcutaneous fat, and free oil was found in the iliac veins and the cavities of the heart.

In October of the same year, Dr. D. J. Hamilton<sup>2</sup> published a case of laceration of the liver, followed by fatty embolism. The patient, a lad, was so little hurt that he was able to walk about, but in an hour or two he became much distressed, his breathing became embarrassed, and coma supervened, death occurring in a few hours. At the *post-mortem* examination the liver, which was peculiarly fatty, was found to have sustained a few small lacerations. The embola were discovered accidentally; they were present in the lungs and kidneys, being more numerous in the lungs.

In the following year, M. Déjérine brought before the Société Anatomique an account of two cases of fatty embolism occurring after fractures. In the first case there had been some hæmor-

<sup>1</sup> *Dorpater Med. Zeitschrift*, 1877, p. 326.

<sup>2</sup> *British Med. Journal*, Oct. 1877.

rhage, the pulse was small and frequent, the respirations were quiet, and death occurred in two hours and a half. The blood contained a large proportion of fat, and the vessels of the lungs were crowded with embola. The absence of dyspnoea and coma is noticeable, and may be held to indicate that death was due to some cause other than the embolism of the pulmonary vessels.

In the second case death occurred in thirty-six hours, and embola were found in the lungs, but the report is very brief, and does not state whether any symptoms were present during life.

In March 1879 M. Déjérine brought the subject before the Société de Biologie, and stated that since the publication of these two cases he had met with ten others in which this condition was present.<sup>1</sup> He found that the number of embola varied with the extent of the injury to the bone. He had made experiments on animals—first by simply fracturing the bones when the amount of embolism was very small, and afterwards by introducing a foreign body into the medullary canal, when the embola became very manifest, and the fat could be followed from the veins of the limb to the lungs. But when a substance capable of expansion, such as a tent of sponge or laminaria, instead of a piece of wood or iron, was introduced into the medullary canal, the lungs became literally injected with fat; and he considered that this indicated the probability that in man fat embolism followed the development of acute osteomyelitis, giving rise to increased pressure within the medullary cavity, so as to force the fat into the osseous capillaries.

At the Société Anatomique, a week later, M. Duret<sup>2</sup> reported a case of compound fracture, followed rapidly by death, in which oil was found in the veins of the limb, and free oil globules in the tissues around the wound; and he remarked, not that this was a case of death from fatty embolism, but that it illustrated the starting point of that condition and the manner in which the fat obtains entrance into the circulation.

Wiener,<sup>3</sup> in the course of some experiments on animals, found that the oil may be absorbed from the serous cavities (peritoneum, pleural sacs), or from the subcutaneous tissue. The

<sup>1</sup> *Le Prog. Méd.*, March 1, 1879.

<sup>2</sup> *Le Prog. Méd.*, March 8, 1879.

<sup>3</sup> *Archiv für Exper. Pathologie*, Band xi.

intervention of lymphatic glands does not prevent embolism. A very high degree of embolism is required to produce death, which is caused by general pulmonary cedema. The phenomena were never followed by any general rise of temperature, or inflammation in the embolised organs. The fat was found in the urine and in the urinary tubules and capillary loops of the glomeruli, so that it is excreted by this channel, according to Wiener, by a process of filtration.

Dr. Egli. Sinclair<sup>1</sup> has stated that fatty embolism was found in 10 per cent. of all the bodies examined in the Pathological Institute at Strasburg. He summarises the causes of this condition as follow:—(1) crushing of fleshy parts of the body, which contain much adipose tissue; (2) lesions of the marrow of bones; (3) inflammatory changes (not acute osteomyelitis) taking place in the latter. The following are the symptoms of fatty embolism:—The patient begins suddenly to feel weak; the respiration rises to about 60; the pulse is small and very frequent; the temperature is high; rattling is heard first in the bronchi, then in the trachea; there is dyspnoea, passing frequently into orthopnoea; the lips are covered with a reddish froth; the face is first pale, and later on becomes cyanotic; the extremities are cool, and the pupils contracted; the patient becomes somnolent, then comatose, and finally dies, death being in some cases preceded by vomiting and convulsions.

Jolly<sup>2</sup> has described three cases in which fat embola were found after simple mechanical rupture of the fat cells of the subcutaneous connective tissue. Symptoms were present during life in three cases only; in these the embola in the pulmonary vessels were very numerous.

In 1880 Mr. Southam of Manchester published a case of double amputation of the lower extremities for injury, in which death seemed to be probably attributable to this cause. Twenty-four hours after the operation the patient became restless and excited; countenance cyanosed, pulse and respiration rapid; there was no dyspnoea nor any rigors. Delirium set in, and death ensued seven hours after the commencement of these symptoms. The temperature at the time of death was 105° Fahr., and in

<sup>1</sup> *Correspbl. f. Schweizer Aerzte*, No. 6, 1879.

<sup>2</sup> *Arch. für Psych.*, Band xi. p. 201.



spite of Listerian precautions, the wounds showed evidences of putrefaction. The lungs were found to contain numerous fat embola. Mr. Southam inclines to the opinion that the actual cause of death was acute septicæmia.

In 1881 Mr. Mansell Moullin<sup>1</sup> published twelve cases of fractures of bones and lesions of soft parts, in which he had found fat embola in the lungs. In only one of these cases were there any symptoms during life to indicate the existence of this condition, and in that case the number of embola was below the average. In one case of fracture of the cervical vertebræ high up, in which death occurred immediately, there was most abundant embolism. He draws attention to the old observation of the occurrence of an oily pellicle on the surface of the urine of fracture patients, and demonstrates the passage of the fat through the glomeruli and urinary tubules. He refers to some experiments by Scriba, who found that in order to produce death it was necessary to inject a quantity of fat equal to three times the weight of the fat contained in the medulla of the femur. He is inclined to the view that pulmonary embolism is usually free from harm, and that when ill effects ensue they are possibly due to embolism of the medulla oblongata.

The following case occurred in the General Hospital, Birmingham, for the notes of which we have to thank Mr. Howard Lowe, the resident surgical officer :—

H. G., a strong healthy looking man, 37 years of age, was admitted on June 21, 1881, with a compound fracture of the left leg, caused by a kick from a horse on the night before admission. He was a good deal addicted to drinking, and was not sober at the time of the accident. He lay by the roadside two or three hours before he was found; his leg was then bandaged up in side splints, and he was driven seven or eight miles to the hospital. On admission he seemed to be in good general health, and was very talkative; the fracture was comminuted, and the wound was filled with blood clot. The limb was placed on a back splint, and carbolic oil dressing applied. The temperature that evening was 101° Fahr. He slept well during the night.

June 22, 10 A.M., his face was rather flushed and dusky, and had a dull heavy look, but he said he felt very well. Tempera-

<sup>1</sup> *The Lancet*, July 30, 1881.

ture 101·4° Fabr., pulse 113, respiration 40. Worried looking, perfectly quiet, blood clot undisturbed. About an hour later he fell into what appeared to be a deep sleep. At noon he was still sleeping heavily, with rapid and stertorous breathing. Pulse soft and dicrotic, face flushed and cyanosed; could be roused with difficulty into a semiconscious state; pupils were slightly dilated while sleeping, and became more dilated when he was roused. There was abundant coarse crepitation at the bases of both lungs. Towards evening the cyanosis increased, and he could scarcely be roused at all. Temperature 101·8°, respiration 42, pulse 14°.

June 23. Cyanosis more marked. Moans, but does not speak when roused. Yawns sometimes. Makes a grimace when his medicine is poured into his mouth. Passes his urine involuntarily.

	M.	E.
T., . . . .	99·8	101·6
P., . . . .	121	114
R., . . . .	...	44

June 24. Cyanosis more marked. Stupor deeper. Pupils equal and rather contracted, but dilate if an attempt is made to rouse him.

	M.	E.
T., . . . .	100·2	104·2
P., . . . .	143	160
R., . . . .	46	56

He died at 10.50 P.M.

At the necropsy the only naked eye appearances were that the brain substance was much injected, there were numerous small ecchymoses on the surface of the heart and lungs, and the bases of the latter were much congested and friable, but floated in water; finally, the large arteries were stained deep red. The fracture was slightly comminuted, and there was much blood extravasated around it, but there was no suppuration. On microscopical examination the lungs were found to be crowded with fat embola, the greater number being found in the small arterioles, few having reached the capillaries; in the kidneys the embola were chiefly in the vessels of the glomeruli, which were sometimes completely injected; there was no fat to be seen in

the lumen of the tubules; but some of the epithelial cells were loaded with minute globules of fat. In the heart the embola were rare.

The appearances seen in the lungs are represented in the accompanying drawing (Pl. XIII. fig. 1).

This case resembles in its chief characters the classical descriptions of death from fatty embolism already quoted, and the appearances in the lungs justify the belief that this was in fact the cause of death.

Since making this observation, we have examined the lungs in a large number of cases dying from disease or accident. Out of ten cases of wounds or injuries, embola were found in eight. The two exceptions were—1. tracheotomy; and 2. herniotomy. The eight in which embola were found were—

- (1) An elderly woman, leg amputated for old standing ulceration. Death twenty-four hours after operation. Heart very fatty. Death probably from heart failure due to shock.
- (2) An elderly man, fracture of the skull, with hæmorrhage from middle meningeal artery. Death occurred in twenty-four hours, from compression by the effused blood.
- (3) A girl, compound fracture of forearm and of both legs, with simple fracture of both femora. Death occurred a few hours after admission.
- (4) A middle aged man, fracture of ribs, and laceration of lung. Death two days after admission.
- (5) A middle aged man, brought in dead, fracture of skull, with hæmorrhage compressing brain.
- (6) A child, simple fracture of femur, with rupture of spleen, kidney, and diaphragm. Death occurred two days after the injury.
- (7) A middle aged man, simple fracture of pelvis and femur; the injuries to the pelvis were very extensive. Death occurred two days after the injury.
- (8) A middle aged man, fracture of femur and tibia, with opening into knee joint, and fractures extending into joint. Death occurred after two days.

In none of these cases were there any symptoms during life which pointed to fat embolism, and in all but two the amount of

embolism found after death was very slight. In two, however, Nos. 3 and 7, the embola were much more numerous, and this excess corresponded to the extensive character of the injuries sustained.

Yet, although in these two the embola were relatively numerous when compared with the remaining six of the series, they were very few in proportion to those visible in the case already described in which death was attributed to this cause.

In the cases of disease no fat embolism was found, with one exception to be referred to immediately. The cases in which no embola were found included a case of diabetes without milky blood, and a case of dysentery.

The exception to which reference has been already made was a case of diabetes with milky blood, notes of which have been published by Dr. Rickards.<sup>1</sup> In that case, the patient, a lad, died rather suddenly, without premonitory symptoms, the terminal coma being preceded by a slight convulsive seizure. At the autopsy the blood was found to be very fatty; when first seen it looked like melted strawberry ice, and on standing the surface became milky white. On collecting some of the blood, and putting it to stand in a conical glass, half its volume became milky white. The heart contained clots of white material like coagulated milk. These were apparently composed of a mixture of fat and fibrine. Under the microscope, the fat was seen to be in a state of extremely fine division, with very few large globules. We extracted some of the fat from the blood by ether, and obtained it again by evaporation. Attempts to stain the fine emulsion with osmic acid failed. The lungs and kidneys were examined for fat embola, and the appearances found are represented in the drawings (Pl. XIII. figs. 1 and 2).

In our opinion these appearances do not constitute embolisms, but are merely due to the presence of fat globules in the coagula which have formed in the vessels after death. It will be noticed that they do not distend the walls of the vessels, as may be seen in the case of death after fracture, but are evidently of less diameter than the lumen of the vessel. Moreover, it is probable that even these globules are to a considerable extent of *post-mortem* formation, due to the running together of the fine

<sup>1</sup> *Birmingham Med. Review*, June 1882.

oil granules when lying side by side and separated from the serum.

The relation of fat embolism to diabetes has assumed a special interest since, in July 1879,<sup>1</sup> the late Prof. Sanders [of Edinburgh, and Prof. D. J. Hamilton, now of Aberdeen, published a paper, in which they contended that the peculiar terminal dyspnoea and coma of diabetes are due to lipæmia and fat embolism; and in support of this proposition they relate a case of diabetic coma in which the blood was fatty, and in which fat embola were found in the lungs and kidneys. These so-called embola are not, however, identical in appearance with those seen in injuries to adipose tissue. If we refer to the plates which illustrate their paper, we find that the oil globules do not as a rule fill up the lumina of the vessels, but appear to be embedded in the thrombi. Moreover, the authors state that the oil drops in the blood, which were at first small, not larger than a blood-corpuscle, had by the following day run together to form large globules. It seems, therefore, quite probable that the large globules were formed after death, and that these so-called fat embola are mere *post-mortem* thrombi with fat globules embedded in them.

Dr. Louis Starr<sup>2</sup> has published a case of diabetes with milky blood, in which so-called fat embola were found. But we are in doubt whether these appearances are correctly described. The drawing shows, in the words of the author, "transverse sections of the small arteries in the larger trabeculæ of fibrous tissue having their lumen congested, and among the blood-corpuscles are seen globules of fat, stained black by the acid." The oblong branching masses are probably similar masses of blood clot containing more or less fat.

Moreover, the retina was examined by Dr. A. G. Heyl, and he has described its appearance. There was no retinal lesion, no hæmorrhages, or embola, but the retinal vessels, veins, and arteries were of a light salmon colour.

Finally, the patient was very much debilitated, and croupous pneumonia was present, sufficient in amount, in Dr. Starr's opinion, to have determined the fatal issue. Here, again, there

<sup>1</sup> *Edin. Med. Journal*, July 1879.

<sup>2</sup> *The New York Medical Record*, May 1, 1880.

was undoubtedly fat in the blood, but not in such a state as to cause true embolism.

Dr. R. H. Fitz<sup>1</sup> has reported a case in which a few fatty embola were found in the lungs; he gives no drawings or details of their appearances, but there can be little question that a "few fatty embola" in the lungs could not give rise to the phenomena of diabetic coma.

Dr. Arthur Gamgee<sup>2</sup> has published an account of two cases of diabetic coma. In the first, analysis of the blood showed it to contain 13·55 parts of fat per thousand; but Dr. Dreschfield was unable to discern any embola in the lungs, kidney, or brain. In the second case the blood, *post-mortem*, was found to contain only 1·88 parts of fat per thousand, a quantity which cannot be regarded as pathological.

Dr. Frederick Taylor<sup>3</sup> states that the viscera of three cases of diabetes dying comatose, with milky blood, have been carefully examined at Guy's Hospital, but no embola were found.

So far, then, there has been very little confirmation of the statements of the Edinburgh pathologists. Hertz,<sup>4</sup> indeed, has stated that milky blood is common in drunkards and in acute pneumonia, but that he has never seen any harm result from it.

It is certainly true that some cases of diabetic coma have not milky blood, and it is only exceptionally the case that those with milky blood present anything that could be called fatty embolism. What we know of the history of fat embolism in fractures warrants our contending that much more decided *post-mortem* appearances are needed to justify our regarding it as yet proved that any case of diabetic coma has been rightly attributed to fatty embolism of the pulmonary vessels.

Dr. Howard Bendall<sup>5</sup> has described the occurrence of very extensive fatty embolism of the pulmonary vessels in acute farcy. The patient suffered from intense dyspnoea, but there was considerable coarse disease of the lung (pneumonia). Both Rayer and Böllinger have described this dyspnoea as of frequent

<sup>1</sup> *Boston Medical and Surgical Journal*, Feb. 10, 1881.

<sup>2</sup> *A Handbook of Physiological Chemistry*, vol. i. p. 169.

<sup>3</sup> *Guy's Hospital Reports*, vol. xxv. p. 158.

<sup>4</sup> *Deutsch. Med. Woch.* 1881, No. 27.

<sup>5</sup> *Proceedings of the Pathological Society, Brit. Med. Journal*, Feb. 11, 1881.

occurrence, but the latter attributes it to the coarser lesions so often present. Dr. Bendall was able to show that the source of the embola was the free oil which was formed in the intramuscular abscesses. When the pus fell into water a considerable quantity of oil rose to the surface, 10-15 minims in an ounce at least. There was besides a rapid fatty necrosis of the tissues around the pustules, and the adipose tissue had broken down, the contents of the cells running together.

This subject requires further investigation, as Mr. Stanley Boyd<sup>1</sup> shortly afterwards described the microscopical appearances in another case of farcy, in which the changes described by Dr. Bendall were not present.

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#### EXPLANATION OF PLATE XIII.

Fig. 1. Fatty embola in lung after fracture. Death by coma. Hartnack, oc. 3, obj. 4. Tube drawn out.

Fig. 2. Condition of pulmonary vessels in a case of diabetic coma with milky blood. Hartnack, oc. 3, obj. 4. Tube drawn out.

Fig. 3. The vessel marked *a* in the previous figure, highly magnified. Hartnack, oc. 3, obj. 8. Tube drawn out.

Fig. 4. Medullary portion of kidney from the same case of diabetes. Hartnack, oc. 3, obj. 4. Tube drawn out.

Fig. 5. Portion of the same, highly magnified. Hartnack, oc. 3, obj. 8. Tube drawn out.

<sup>1</sup> Pathological Society's Proceedings, *Brit. Med. Jour.*, April 15, 1882.

THE BOSTON  
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MICROCOCCLUS POISONING. BY ALEX. OGSTON, M.D.,  
*Surgeon to the Aberdeen Royal Infirmary.*

IN former publications upon the connection between micro-organisms and acute inflammatory processes,<sup>1</sup> I have shown,—1st, that acute inflammation is capable of being produced by micrococci; 2nd, that they are the cause of acute suppurative inflammations in man; and, 3rd, that there is a close relationship between acute inflammation, suppuration, and the various processes that we term pyæmic and septicæmic. A considerable time has elapsed since the report left my hands, and during that period a good deal has been written that bears on the questions there raised, and tends either to confirm or contradict such views as those there expressed. It is therefore desirable, and it will be found to be in the long run conducive to clearness, that the facts and arguments of the former report be here briefly restated and commented on.

It was found, in an examination of a hundred abscesses in man (a number that has been considerably increased since the report was published), that every one (save thirteen that were "chronic or cold abscesses") contained micrococci mingled with the pus, usually unmixed with any other micro-organisms, but sometimes (in 12 per cent. of the cases) containing other forms, bacillus, bacterium, or spirillum. It was next discovered that the pure micrococci from these abscesses produced, when injected in a dose not too small, similar abscesses in animals, whether injected as they are found in human pus, or after cultivation in media removed from the influence of atmospheric air. It was observed that "chronic" pus, where these organisms did not exist, produced no similar effects, that the action of heat or disinfectants rendered the "acute" pus innocuous, and that the pure micrococci, cultivated in eggs to the one hundred and forty-six millionth dilution, produced the same inflammation and abscesses. This clearly proved that the micrococci were producers of suppuration, and the microscopic examination of the places where they had been injected into animals afforded evidence of the correctness of this by revealing their proliferation there, their formation of the first inflammatory knot, their peripheral invasion of the tissues, their passage into the blood during the stage of fever, and their final excretion from the

<sup>1</sup> Report on Micro-organisms in Surgical Diseases, *Brit. Med. Jour.*, March 12, 1881, p. 369; and Preliminary Report, entitled "Ueber Abscesses," *Archiv f. Klin. Chir.*, 1880, Bd. xxv., and read to the German Surgical Society, 9th April 1880.



system by the formation of a delimiting wall of granulation tissue that arrested their invasion, and led to their final extrusion amongst the pus.

These facts have been contradicted with more or less plainness, or other explanations of the inferences have been suggested. It concerns me to draw attention to these.

In October of last year Uskoff,<sup>1</sup> of Cronstadt, under the auspices of Professor Ponfick, published a series of experiments on animals consisting of subcutaneous injections of bland liquids (water, milk, or oil), of irritating liquids (turpentine, turpentine with oil, or carbolic acid with turpentine or water), and of pus, and the results they showed in the production of suppuration and inflammation. His paper has received some attention, especially in English medical periodicals, as in the *Lancet* of 25th October 1881, and the *British Medical Journal* of 26th November 1881, where it is quoted as contradicting the theory that acute suppuration depends on the presence of micro-organisms.

This, however, it does not do. Uskoff's conclusions, given at the close of his paper, are that micrococci and bacilli are admittedly the producers of suppuration in many, though by no means all, cases; and that strong chemical agents can, without any organisms, produce the most intense suppurations.

Uskoff's facts will well repay a short review, and it will be found that they do not in the smallest degree shake the position taken up in last year's report, but, on the contrary, support it in every point save where the reason for their divergence is clear.

I assume Uskoff's microscopical observations to be correct, although the optical means he used are not mentioned, and, as I shall afterwards show, this is a vital point in all observations regarding micro-organisms.

Uskoff injected into the subcutaneous tissue of dogs, sometimes repeating the injections at intervals of two to six days, bland liquids, such as water, milk, or oil, purified as far as possible by boiling and subsequently cooled. He used water in six, milk in four, and oil in eight dogs, in all eighteen, experiments. Of these, two experiments with water and six with oil caused no detectable suppuration; while the remaining ten cases, where suppuration occurred, all showed the presence of micrococci, with bacilli in addition in two. In some of these (six cases) he records that very few micrococci were present, not a surprising statement considering that in four the suppuration was very slight, while in the other two where the amount of the suppura-

<sup>1</sup> Virchow's *Archiv*, vol. lxxvi. p. 150, 1881

tion is not indicated, the quantities of liquid injected had been 300 and 220 grammes respectively, or more than 9 and 7 fluid ounces, a quantity that would readily dilute the pus and all it might contain.<sup>1</sup>

Uskoff in one case (his second one) says that the micro-organisms "could not be recognised with absolute certainty," a very strange observation, as will be afterwards seen, if he used the proper optical means for detecting them. But with every qualification the result of his injections of bland fluids came to this: that *wherever they produced suppuration micrococci were present, while no micrococci were found where they caused none.*

His second set of experiments consisted of subcutaneous injections, in five cases of turpentine, in four of oil with turpentine (in proportions of 5 : 1 and 10 : 1), and in three of carbolic acid, diluted in one case with 5 parts of turpentine, in another with 2½ parts of turpentine, and in the third with 20 parts of water. His irritating injections were thus twelve in number, the last being merely the ordinary "one to twenty" carbolic lotion. As might have been expected, this last produced no suppuration, and thus there remain for consideration eleven experiments, in all of which turpentine was injected, undiluted in five, and in the others not weaker than 1 : 10. Where injected pure it caused extensive necrosis and abscess, without micrococci being present; and where it was diluted it caused suppuration in four cases, produced necrosis in one, and was without result in one, but no organisms were present in any save perhaps in one, concerning which Uskoff is again doubtful. Uskoff remarks<sup>2</sup> concerning this series,— "We may safely conclude that in these experiments the production of suppuration is attributable to the turpentine."

No one is likely to disagree with this; turpentine will surely produce suppuration, and without experiment the same thing may with the highest probability be predicted of many another chemical irritant, such as a strong acid, a strong alkali, the strong caustics like chloride of zinc, and many others, if injected in quantities of an ounce and more, as done by Uskoff. But that has little to do with the suppurations we meet with in nature. Nature never injects turpentine or other caustics into our bodies in quantities of an ounce and upwards, and until the usual processes of disease are changed it will still be correct to say that micrococci are the producers of acute suppuration, whatever be the result of such experiments as these. It certainly was not the intention of the previous report to convey the

<sup>1</sup> There is, however, an additional consideration that concerns the occurrence of only a few micrococci in some abscesses that will afterwards have to be dealt with, and that also has its bearings on Uskoff's experiments.

<sup>2</sup> And possibly the belief that his experiments contradict or correct mine may have been induced by a hasty consideration of this passage.

meaning that nothing else would produce abscess saving micrococcus. On the contrary, it was expressly emphasised that *chronic* abscesses contained no organisms, and therefore were not due to micrococcus. Several causes are known to produce suppuration, the breaking down of a part in a state of fatty or inflammatory degeneration, as in the cheesy foci of glands and bones; possibly also the occurrence of great tension in cavities distended with an effusion or in tissues infiltrated with blood or other liquid, a cause often urged by Mr. Lister, and with which we must all to some extent agree, although it is evident that mere tension does not usually lead to suppuration, a fact well shown in advanced cases of hydrocele, ascites, and ovarian dropsy. Chronic and acute suppurations are identical as processes, but are due in the former to a less irritating and in the latter to a more irritating cause. No other difference exists between them, and the revelations of recent years enable us to form a tolerably accurate surmise concerning the exact mode in which each is produced.

The researches of Köster, König, Volkmann, Friedländer, and others, regarding tubercle and caseous disease, demonstrate clearly that chronic suppurations are the specific evidences of local manifestations of tubercle.<sup>1</sup>

Brouardel and Boutmy,<sup>2</sup> on the other hand, and the investigators who in recent times have in such numbers occupied themselves with questions of decomposition and fermentation, have made it clear that all micro-organisms, as well those that cause ordinary decomposition (saprophytes) as those which produce decompositions not identical with putridity, generate, in the process of living, changes in the substances where they grow; or, in other words, that in feeding on the soils they live in, they leave residues whose atomic constitution is so changed that new substances are presented to us in them, some of them virulent poisons, some of them gases, some offensive, others not so, and that these *ptomaines*<sup>3</sup> or cadaveric compounds vary according to the

<sup>1</sup> Rheumatism causes inflammation which never suppurates, as in acute articular rheumatism; gout does the same; tubercle causes inflammation ending in chronic suppuration; micrococci cause inflammation ending in acute suppuration. Inflammation is but a modification of tissue growth, and variations in its nature are due to variations in its cause.

<sup>2</sup> Sur le développement des Alcaloïdes cadaveriques, *Annales d'hygiène publique*, No. 22, p. 344; also No. 31, p. 9, 1881.

<sup>3</sup> Throughout this report the word *ptomaine* (πρωμα, a carcase) is used to signify any poisonous or noxious substance generated during the growth of micro-organisms, a sense greatly more extended than that in which it is employed by Brouardel and Boutmy.

organism and the soil where it may be growing. Hence we learn that just as alcohol is a ptomaine produced by the torula, so the smaller micro-organisms have their special ptomaines, some of these being the gases of decomposition, and others gaseous, liquid, crystalloid, or colloid substances that have compositions and properties concerning which we are as yet ignorant.

It will not occur to any one familiar with the literature of bacteria investigation to deny that ordinary forms of micro-organisms furnish poisonous ptomaines; for this is made evident by the results of injections into animals of septic fluids, out of which all the organic constituents have been filtered in passing through an unglazed porcelain cell.

It is not necessary to quote such experiments at length here, but I may refer, as it happens to lie to my hand to do so, to the poisonous results obtained in this manner by Wolff<sup>1</sup> from the liquid part of pus, and to the well-known experiment of Koch,<sup>2</sup> who found that a large dose of a putrid fluid subcutaneously injected produced rapid death from poisoning before the organisms injected with it had time to grow, while a smaller dose produced death in the converse manner of slowly killing by their multiplication. As far as I can profess to understand the matter, it seems unreasonable to think that micrococci cause suppuration by their mechanical effects; it must, on the contrary, be by the ptomaines that they produce that they irritate the tissues, and cause the inflammation and suppuration.

I cannot imagine a great number of mere unirritant molecules producing inflammation and suppuration, such as the micrococci can be experimentally shown to produce; but if it be admitted that they produce irritating ptomaines, the matter becomes at once clear.

In last year's report it was noted<sup>3</sup> that the tissues around the colonies of the micrococci presented a vitreous or waxy appearance, as if cauterised, and it may be looked upon as being far from unlikely that the very reason why micrococci produce suppuration is that they, in growing among the tissues, generate some acrid ptomaines that may correspond pretty closely in their effects with those of injections of turpentine, or other caustic liquid.

It is in truth irrational to speak of one cause as giving rise to

<sup>1</sup> Virchow's *Archiv*, vol. lxxxi. p. 255.

<sup>2</sup> *Wundinfektionskrankheiten*, 1878, by Dr Robert Koch, p. 40.

<sup>3</sup> *Brit. Med. Jour.*, 1881, vol. i. p. 371.

the process *suppuration*, and another as producing mere *inflammation*. Suppuration cannot be rightly understood, save in so far as it is regarded as one stage of inflammation,—an intense stage. Since suppuration merely shows that the inflammation has been intense, so any cause that produces a sufficiently violent inflammation is of necessity a cause of suppuration. Suppuration cannot be altogether disjoined from inflammation. It would be no easy matter now-a-days to discover an individual unaware that a scratch of a pin, a drop of an acid, or a crumb of mustard, will produce inflammation on the interdigital web of a frog; that exposure to air will inflame the mesentery of a mouse, or that the application of cantharides will produce an inflammation of the skin. Some irritants, such as croton oil and tartar emetic, are universally known to produce pustules, *i.e.*, suppuration. Whoever knows these facts knows that there are influences other than micrococci that will cause inflammation sometimes so intense as to end in suppuration, and in claiming that micrococci are the cause of abscess and acute inflammation, I can hardly have misled any one into supposing that such substances as croton oil, if injected subcutaneously, would not likewise give rise to pus.

But subcutaneous injection is an artifice, and artifice has nothing to do with the action of nature's laws in producing acute inflammations; so that, seeing that turpentine does not circulate in our bodies in quantities of upwards of an ounce, I hold it is, for all that *Uskoff's first and second sets of experiments have shown, rational and correct still to say that micrococci are the producers of acute inflammation and abscess in man.*

To avoid any misunderstanding, however, I shall here append the qualification which I coupled with this statement in the former report, *viz.* :—

"It is not intended to be conveyed by anything that has been said that micrococcus is the only organism which produces such results."

Coming next to *Uskoff's* last set of experiments, *viz.*, his *injections* of pus, we find that he injected it six times in quantities of a dram or two, *but never once injected pus that contained micrococci* so that the experiments are worthless for showing whether or not micrococci will produce suppuration. On the contrary, three of his injections were with the pus obtained from his former experi-

ments with undiluted turpentine, and the pus smelt still of turpentine, as he expressly mentions. Two of these caused abscesses with bacilli among the pus, and one produced no result. His fourth injection was with the same turpentine pus after standing for four days, during which time it lost its odour of turpentine and developed bacilli; while his remaining two experiments were injections of the pus, containing bacillus, that resulted from injecting the terebinthinous pus in the first two of the present set of experiments, and both were without effect. The inferences, so far as any can be drawn from this set of experiments, seem to me clear, and are—first, a liquid containing dissolved turpentine will produce abscess (which was already known); and, second, pus containing bacilli, but no micrococci, does not produce abscess. But as regards the question of what influence micrococcus has in causing suppuration, they have no significance whatsoever, either positive or negative.

My other critic is one with whom I am right loath to disagree, although I know well that Professor Lister will think no evil of those who contradict him in the search after truth. Mr. Lister<sup>1</sup> combats the idea

“That all inflammation is caused by micro-organisms, and that suppuration, whether acute or chronic, is always due to similar agencies.”

I agree with this. All inflammation is not caused by micro-organisms, and chronic suppuration has not been shown to have anything to do with them; but I feel bound to add that acute inflammation, so acute that suppuration is present or imminent, is, so far as I can form an opinion, always due to them save in the exceptional cases where a burn or blister, or some similar cause, has been at work.

But Mr. Lister goes further, and in support of the thesis that an acute inflammation may be present without micro-organisms, cites eight actual cases or well-known occurrences, viz.:—

1st. Suppression of urine, uræmia, intensely congested kidneys, and death from a urethral irritation. 2d. Urethral pain, rigor, and suppression of urine from the passage of a bougie.

<sup>1</sup> *Lancet*, 22d October 1881, p. 695. Address on the relation of micro-organisms to Inflammation in the Pathological Section of the International Medical Congress.

I have had the same melancholy experience of such cases as other surgeons, and have had occasions to examine them *post-mortem*, and I feel bound to ask what evidence is there that these cases are inflammatory? It nowhere exists, and it still remains to be shown that they have anything to do with inflammation.

Mr. Lister cites 3rd, a redness of the skin from tight stitches in a wound, that faded in a few minutes after the removal of the stitches; 4th, an oesophageal irritation from lodgment of a morsel of food that produced anorexia until removed; 5th, a sub-acute effusion into the knee-joint produced by a fall; 6th, a sore throat; 7th, a painful affection of the shoulder-joint that had all the clinical characters of neuralgia, and none of inflammation; and, lastly (8th), a peculiar affection of the neck about the atlas, causing a prominence in the throat with intense pain along the nerves, and named "ulceration of cartilage."

But not one of these cases, save the sore throat, was a case of acute inflammation verging on suppuration, and it does not seem to be denied that the throat inflammation may have been caused by micro-organisms. These cases have merely to be read over for any one at once to see that they are not cases of acute inflammation at all, that even the shoulder case was but a neuralgia, and the atlas case merely pain from pressure on nerves in *chronic* disease of the vertebræ. Therefore they have no claim to be taken into account in deciding concerning the province of micro-organisms in inflammation, although they may have a bearing upon the use of counter-irritation, a subject that Mr. Lister carries along with him through the whole of this portion of his address at the Pathological Section of the International Congress.

Mr. Lister holds that if "counter-irritation be a powerful means of treating inflammation, and if the explanation" he gives "of the mode of action of counter-irritation" <sup>1</sup> as illustrated by physiological cases, is correct, then the effect of counter-irritation in the treatment of inflammation throws great light upon the nature of inflammation itself, and upon the relation of the nervous system to it. If counter-irritation cures an inflammation by withdrawing nervous action from the affected part, it follows that the inflammation so cured was maintained by an abnormal action of the nerves of the part."

Now, I have pointed out that none of Mr. Lister's cases were inflammatory; nay, I will even go further, and add that I know

<sup>1</sup> Viz., that it is due to nervous action.

of no satisfactory evidence anywhere existing that would lead us to believe that counter-irritation has any influence on inflammation; and I hope Mr. Lister will not think I am overstepping the bounds of fair criticism, when I say that the amount of true scientific evidence as to the causal connection between micro-organisms and acute inflammation is very great, while similar evidence as to the efficacy of counter-irritation does not exist, and, therefore, I cannot abandon the position I have taken up concerning the dependence of acute inflammation on micro-organisms, but I must him to reconsider his position, that counter-irritation is of use in inflammation.

It appears that the brevity that was studiously aimed at in the former report has prevented full justice being done to the views regarding inflammation there given out, for in the same address Mr. Lister offers criticisms that are, I fear, the result of my failing fully to explain the inferences that should be drawn from the facts I collected regarding micrococci.

Mr. Lister asks, "how does a chill of the surface lead to an internal organ becoming inflamed? Why should a draught of cold air upon the chest lead to an attack of inflammation of the lungs? First, it seems clear that some way or other the effect is brought about through the nervous system, for only the integument is chilled, and the deeply seated internal organ suffers, while the same law of sympathy or nervous connection which we have seen in counter-irritation applies equally here: it is the cooling of the skin nearest the internal organ that is most likely to cause its inflammation. And, further, the cold sometimes 'strikes in' with a suddenness that conveys the idea of a nervous shock, and seems to preclude the hypothesis of any other agency than that of the nervous system." And, further on (page 697), "when we turn to acute abscess, it seems to me that Dr. Ogston leaves us entirely without any explanation as to the origin of the infection in the part in which the abscess occurs. If we are to suppose that the micrococci are really the cause of the suppuration, we must also suppose them to be the cause of the inflammation which precedes it. But the inflammation that precedes the suppuration may be induced by some altogether accidental circumstance. For instance, a woman during lactation, with the mammary gland in a state of high physiological activity . . . is disposed to 'take cold' in the part; and as the result of an accidental chill an acute attack of inflammation may occur, threatening milk abscess. If we get such a case to treat in the early stages, the inflammation may never go on to suppuration at all; it may terminate by resolution. But if left to run its course it causes abscess. We can hardly suppose that the accident



of exposure to cold could lead directly to the development of micrococci in the part. Nor even if this were admitted, can we readily understand how any treatment that we can adopt could lead to their dispersal if they were the essential cause of the inflammation;" and yet further, "the micrococci are, so to speak, a mere accident of these acute abscesses, and . . . . their introduction depends on the system being disordered."

Now in these remarks there is much that is open to challenge. Mr. Lister sees in everything evidences that the nervous system is at the root of inflammation, and, as it seems to me on very insufficient grounds. Of course, I do not deny that it plays a very important part in inflammation; I will even go so far as to say, that without it inflammation would be a different process; but all this is equally true of the vascular system, or of the tissues themselves. All three are important, and the complex process that we call inflammation is a process to which each contributes a nearly equal share. But I am ignorant of any facts that afford a warrant for the view, that the nervous system ought to be selected as the one of leading importance; all that physiologists, pathologists, and clinicians have collected to build up our views of inflammation points much more strongly to the importance of the vascular than of the nervous system, while Mr. Lister's own beautiful observations, that go nearer the root of the matter than any others, point to the influence of the tissues being pre-eminent and fundamental. Yet who could logically call either the vascular or the tissue system the cause of inflammation? It would be as proper to term the machinery of a steam-engine the cause of its movement.

The hypothesis that inflammation is caused by the nervous system, in the absence of any undisputed facts to support it necessitates on Mr. Lister's part the adducing of some considerations that seem to me to be even erroneous. A chill of the surface, a draught of cold air, is assumed to be capable of causing an inflammation of the lungs; and an accidental chill, a mammary abscess; while it is inferred that the connection can only be through the nervous system. But, I must ask, *is it true* that the chill will produce these effects, and where is the evidence? So far as is known to me, no evidence whatever exists; no series of observations, no sets of experiments, nothing that possesses any show of cogency can be adduced in proof, but

merely a belief in the connection between a chill and an inflammation that is common enough, but that can readily be shown to be erroneous. If we examine nature as to the connection between cold and inflammation, we find that exposure to cold, whether sudden or gradual, short or prolonged, by no means gives rise to inflammation except where actual congelation is produced, when there is a reactive inflammation of the portion frozen more or less violent in proportion to the intensity or duration of the congelation, but there are no remote or reflex inflammations, no pneumonias produced. Exposure to cold does not even produce a phenomenon identical with a "chill;" it indeed calls forth the nervous disturbance called shivering, a disturbance that also exists during "a chill," but this is all they have in common. The exposure to cold produces *a diminution of temperature and a lowering of the forces of the tissues*, that are dangerous to life in proportion to their intensity and duration, but that do not produce inflammation with any such frequency as to afford grounds for believing that the one is directly the cause of the other. All our clinical experience, too, is against the correctness of Mr. Lister's idea that the chill, acting through the nervous system, produces inflammation, for cooling of the skin is not at all likely to cause inflammation of the deep part most near to it; there seems, on the contrary, little connection between the part exposed and the part attacked. If mammary abscess were due to a chill, why should it almost always occur about a week or ten days after labour, when the patient is as a rule careful to avoid chills, and seldom during the many subsequent months of lactation, when the exposures are greater and more frequent?

Now all the facts to be adduced in support of Mr. Lister's hypothesis bear out the supposition of a bacteric infection when they are correctly put. Exposure to cold is a cause of inflammation (though in a different sense from that in which Mr. Lister states it), when by reducing the forces of the tissues it diminishes the power of the system to resist the intrusion of micro-organisms. Micro-organisms exist in myriads in our intestines; every drop of liquid there contains millions, but they do not penetrate into the tissues of a strong animal, they remain in the cavities of the intestines. Yet they

are always ready to take advantage of any weak point, and the vicinity of any disease (such as an abscess) to the intestine suffices by its action in weakening the wall of the bowel to allow all the forms of micro-organisms that exist in the bowel to traverse the intact though weakened wall, and appear in the pus, a thing that does not occur save in this situation, and which is the reason why abscesses near the intestinal tract are always foetid. In the same way advantage is taken of lowered states of health, such as are produced by, among other causes, exposure to cold, and organisms that are not present in the blood and tissues of a strong individual in health, may circulate there in states of greater or less activity, after the temporary depressing influence of cold, or after any other influence that acts as a depressant, such as hæmorrhage, fever, over-fatigue, or excess in eating or drinking, all of them conditions liable to favour the occurrence of inflammation or other bacteric diseases.

Thus Kocher<sup>1</sup> has shown that foul feeding will convert a traumatic osteo-myelitis, that was healing kindly, into a virulent osteo-myelitis by this absorption of micro-organisms from the intestines; and Blaschko<sup>2</sup> has shown that febrile conditions are coincident with the presence of micro-organisms in the circulation and in organs where they do not occur in health.

The depressing power of cold, then, like other influences, by diminishing the forces of the tissues, allows the immigration of micro-organisms from the intestinal tract as well as through other channels, in a way that does not occur in health; and it occasionally happens that these, though limited in their malign influence by the energy remaining in the blood and tissues, find a weak spot where they may unfold their adverse influences. Some time, however, is required for their doing so, and it seems more than probable that they have effected a lodgment, infiltrated and proliferated to some extent, ere they reveal their presence by noticeable signs. But they cannot long be con-

<sup>1</sup> "Zur Aetiologie der acuten Entzündungen," by Theodor Kocher in Bern, *Archiv für Klin. Chir.*, 1879, Bd. xxiii. p. 101.

<sup>2</sup> "Ueber Veränderungen im Gehirn bei fieberhaften Krankheiten," by Dr. A. Blaschko in Berlin, *Virchow's Archiv*, 1881, Bd. lxxxiii. p. 471. I am sorry that Mr. Watson Cheyne's experiments on the influence of phosphorus in causing invasion of the tissues of animals by micro-organisms, quoted by Mr. Lister, are not at my disposal, and are known to me only through Mr. Lister's address.

cealed: the inflamed spot is perhaps suddenly detected, it may be during some rapid movement of the individual, and then we are told that a strain or a twist has caused the inflammation; or it may be that the micro-organisms, passing into the circulation and showering their ptomaines into the blood, cause a rise of temperature, and then, *some time, hours or days, after the exposure to the cold, the sensation of shivering, or rigor, or "chill,"* due to an elevated temperature of the body, and indeed one of the usual symptoms of a rise of temperature, *is perceived, while the pain draws attention to the inflammation, and the chill is blamed for producing it.* This, in my experience, is the order of events, instead of that which Mr. Lister describes. I find first the depressing influence, then a period of latency, and then the chill and the local pain. I have directed much attention to observing such matters during the last five or six years, and I am convinced that clinical investigation will bear out the correctness of this.

In mammary abscess too, what have we? I have shown<sup>1</sup> that from the ninth hour of delivery onwards the lochial discharges of the female are crowded with all forms of organisms in exceptional richness and luxuriance; and what more reasonable than the supposition that some of these, obtaining an entrance into the circulation in the state of altered tissues and lowered vitality that follows on pregnancy and childbirth, though powerless to effect a lodgment in the stronger organs, should yet colonise the weakened and congested mamma, and after a few days of unnoticed growth and incubation, should reach a pitch at which the feeling of chilliness and local pain would be well accounted for by what was occurring. In this way too it can reasonably be explained why a mammary abscess occurs at the customary period after childbirth, because the lowered vitality and the poisonous lochiæ coincide to favour its production, as also why the absence of these conditions furnishes a reason for its rarity at other times, when it ought to have been most common, if Mr. Lister's views were correct.

In a similar manner may the occurrence of abscesses and inflammations after fevers be explained in a way that agrees well both with clinical experience and with the views I have advanced regarding inflammation.

<sup>1</sup> *Brit. Med. Jour.*, 12th March 1881, p. 372.

On another point in Mr. Lister's address I must touch. He says—

"I opened a milk abscess and introduced a drainage tube antiseptically, and there was not another drop of pus after the evacuation of the original thick contents, merely a serous oozing, and this rapidly subsided. Why did not the suppuration continue if the micrococci were the essential cause of it? I examined, along with Mr. Cheyne, some preparations which he was good enough to make for me, by Koch's method, of the serum that came from this abscess three days after it had been opened, and we had to make a long search before we could discover a single group of micrococci."

Now I have shown that micrococci grow luxuriantly in the tissues forming the focus of the inflammation *for a time*, and infiltrate the portion until it suppurates, but that when the individual is strong enough to withstand their advance, a wall of granulation tissue is formed around them, limiting their advance and cutting them off from the system; that the central spot of invasion breaks down into an abscess, surrounded by a wall of granulation tissue that resists strongly the encroachments of the micrococcus, and *on the inner surface of which they grow but feebly and imperfectly*. I have shown that an abscess increases and points, not by a further infiltration of micrococci, which probably never grow very much after they have once been limited by granulations (a matter I shall have to touch upon at a later stage of this paper), but by the tension of the fluid it contains (a matter to which I attach nearly as much importance as Mr. Lister), and perhaps the acidity of its contents, causing a rapidly increasing effusion of liquid into its cavity, a consequent increase of bulk, an advance of the granulation wall to the surface, and finally a thinning ere it bursts and allows its contents to escape. I have also shown that micrococci do not grow well when exposed to the air, and I quite disagree with Mr. Lister in expecting that in an opened and drained abscess surrounded by healthy granulations and in free communication through the drainage tube with the air, they should continue to grow with any luxuriance or produce any effects on the tissues around them.

If Mr. Lister had injected one drop of the original thick pus into the back of a mouse, *i.e.*, placed the micrococci under proper conditions, he would have found them invading and infiltrating

(just as if he had inoculated the matter from a soft chancre), until the formation of a granulation wall again limited their activity, and led to their sequestration in the form of an acute abscess. If, on the other hand, he had injected ten times the quantity of pus from a chronic abscess, no such result would have been obtained, but the matter would have been absorbed in a day or two. How does this agree with the idea that micrococci accidentally get in, and finding a congenial soil in the pus, develop there in abundance? How explain why large quantities of albumen may be subcutaneously injected with impunity, but if it contain properly cultivated micrococci, a drop of it immediately gives rise to an abscess? The micrococci cannot be supposed to cause the different behaviour of the two fluids unless they themselves have some irritating properties, some acrid influence that offends the tissue, unless, in other words, they can cause inflammation, which is the point on which I insist. Why does every acute abscess contain them, while no chronic abscess shows any? What answer can be given but the straightforward one, that they are the cause of the difference? And if experiments with fluids that contain them and fluids that do not, show that the former alone produce abscess; and if, further, they be cultivated in an innocent fluid, like white of egg, till they are isolated from all else that surrounded them, and the same results be again obtained, what inference can be drawn but that they are the cause of the suppuration?

It is true there are difficulties to explain, but the explanation is easy and natural, while any other supposition, such as that nerve influence causes inflammation and the micrococci are a mere accident, requires such an amount of unproved assumption that it cannot be allowed. The chief difficulty concerning the micrococci is, that they are often found in a form that does not readily cause inflammation. If micrococci are obtained from the water or the atmosphere and cultivated with access of air, they may be injected into the tissues with impunity, and those that are virulent, as in pus, may, by a like treatment, be rendered harmless. But this is not surprising; on the contrary, it agrees with what we know to be true of micro-organisms in general.

Has not Grawitz<sup>1</sup> shown that fungi that are innocent may be

<sup>1</sup> Virchow's *Archiv*, Bd. lxxxi. p. 361.

cultivated so as to become deadly in their virulence; has not Buchner<sup>1</sup> obtained the bacillus of anthrax by cultivating the innocent hay-bacillus, and what is this but a true analogy to the common harmless micrococci becoming under suitable conditions the virulent causes of inflammation? Have not Pasteur, and Toussaint, and Boulay<sup>2</sup> shown that the poisonous organisms of anthrax, fowl-cholera, and bovine pleuro-pneumonia may be weakened so that their deadly nature is changed, and they are even converted into a protective vaccine? and has not Buchner<sup>3</sup> converted the fatal anthrax bacillus back into the harmless hay organism?

In the face of all that, is it difficult to believe that micrococcus can be injurious or innocent, just as it is allowed to grow under suitable or unsuitable conditions of culture or soil? My conviction is, that the common micrococci that exist around and on us and in our intestines—that are resisted in their attempts at entrance by healthy individuals, but sometimes enter under conditions of lessened vitality—are one and the same with the virulent cocci that cause inflammation; that the difference is only that the production of acute inflammation shows they have found a suitable soil, a weak spot, or a weak individual, where they are cultivated in a mode suited to elicit their virulent qualities. The extent and intensity of their virulence is only a question of the amount of soil at their disposal, and of the extent to which the resistance of the individual has been diminished.

Another difficulty in the way is the resistance to development of micro-organisms by the forces of the tissues. That such exists is beyond doubt: it is as well established as any fact in medical science. We know that though micro-organisms line the surfaces of our bodies, yet they are never found in their interior in health.

It is demonstrable that we dare not attach much importance to experiments concerning the matter made on animals, where the conditions have never been entirely satisfactory, and where the results have always been open to question. But every observer who has worked with processes of actual observation of

<sup>1</sup> *Milzbrandcontagium*, 1880; *Sitzber. d. bayer Acad. der Wissensch.* (Reprint), p. 394.

<sup>2</sup> *Bulletin de l'Acad. de Med.*, 1881, No. 36, "Inoculation préventive de la peripneumonie contagieuse des bêtes bovines."

<sup>3</sup> *Milzbrandcontagium*, p. 382.

the blood and tissues of man is agreed as to the absence from them of micro-organisms, save in disease. No one has ever demonstrated an organism in healthy blood—none are ever found in healthy urine—none in the ordinary serous effusions of chronic and subacute inflammation, such as hydrocele, hydrothorax, ascites, or bursitis; and none are ever seen in the aqueous humour, or in the vitreous body of the eye. I have also minutely examined the tissues and contents of the intestinal tract in an infant that had died in utero and was born by miscarriage at the seventh month of utero-gestation, but failed to find a single micro-organism present. We further know that if organisms be introduced into the blood and tissues they die, save when the latter are unhealthy or weakened, or where the quantity introduced is too great for the forces that are at work to destroy them. We know that nearly every form of weakness permits their entrance to some extent into the system, and that they again disappear with returning health, while in the weakness that results from approaching death from any cause they enter the system freely and almost unopposed, giving rise to incipient changes and decomposition that commence the work of putrefaction even before life is extinguished.

The facts regarding the resistance of healthy tissues are certainly extraordinary, but they do not admit of denial, and they correspond with what clinical observation of disease teaches us. We know, for example, that the purely zymotic diseases attack some individuals severely, while others are but slightly affected, and some entirely escape; that the stronger the individual the less likely is he to be attacked by such diseases as diphtheria, erysipelas, and abscess, while those weakened by residence in an unhealthy or unsanitary locality are prone to be seized with severe forms; that individuals weakened by loss of blood are prone to all forms of blood-poisoning; that acute diseases which weaken an individual are apt, even though they are themselves bacteric (zymotic), to be followed by abscesses and inflammations; that women after pregnancy and delivery, whose tissues are altered and weakened in the way known to gynaecologists, are specially prone to puerperal fevers, pelvic inflammations, mammary abscess, and septic forms of endocarditis; and that some weakly individuals are predisposed to erysipelas. A list



of clinical facts pointing in the same direction might be almost indefinitely extended. But enough has been said to show that a perfect understanding of the relations between bacteria and the system cannot be attained until we have arrived at a definite knowledge concerning this peculiar influence of the living tissues that is so hostile to the life of micro-organisms.

A further difficulty in the way of defining their relations to inflammations consists in the want of definitions regarding inflammation. What is inflammation, what is chronic, or subacute or acute inflammation? I have shown that Mr. Lister considers suppression of urine and renal congestion following on irritation of the urethra inflammation, while to my mind it is an entirely different process, presenting none of the usual signs, and having nothing in common with it save congestion and impairment of function. Mr. Lister, too, looks on a bursitis as an acute inflammation, while to my mind it is of a different nature—due at first to an injury, a subcutaneous laceration or contusion, causing effusion into a sac, where the tension of the effusion keeps up a condition of sub-acute inflammation that will never become acute, never suppurate unless micrococci or some similar agency be introduced into it. Tension is, to my mind, incapable of causing acute inflammation. I cannot recall one of the numerous cases I have seen of tension in comminuted fracture, or in advanced ascites, ovarian dropsy, or hydrocele, that ended in acute suppurative inflammation; and hence, while I readily admit to tension the power of keeping up chronic inflammation, and even allow it a certain share in the phenomena of acute abscess, I cannot convince myself that it has any necessary connection with acute inflammation, such as I claim to be due to micrococcus.

Nor does any condition produced by the nervous system (with one exception) seem to me capable of being fairly called acute inflammation. None of the conditions Mr. Lister has cited is capable of sustaining his thesis that inflammation is caused by the nervous system, and his own researches (especially the experiments,<sup>1</sup> where a limited area of a frog's foot was inflamed by a crumb of mustard, and the blood that reached it in the healthy vessels showed, in passing through the spot, corpuscular

<sup>1</sup> *Trans. Roy. Soc.*, 1859, vol. cxlviii. part ii. p. 660 *et seq.*

agglutination and clinging of the leucocytes to the walls, while beyond that area it was normal) are absolutely contradictory of any such view. The one disease that might be quoted in his support is herpes, especially herpes zoster, believed to be the result of a diseased condition of a ganglionic centre; but the evidence as to the connection between the nerve disease and the eruption must be better defined and established before the validity of this solitary instance can be admitted.

Perhaps the use of the word *phlegmon* might aid in defining my position. It will be admitted by all that phlegmon implies acute inflammation, and I think I may also assume that all phlegmons have a tendency to end in suppuration. If the word *phlegmonous* were held to be equivalent to *tending to suppuration*, we could then speak of phlegmonous blisters, phlegmonous inflammation of serous membranes, phlegmonous pneumonia, adenitis, bursitis, &c. If the word be used in this sense, then the assertion that micrococci are the producers of phlegmons and phlegmonous inflammations comes very near to what I intended to imply by the assertion that micrococci are the cause of acute inflammation. But I again expressly state that I am by no means convinced that other organisms besides micrococci will not produce acute inflammation, nor do I assert that no other agencies save micro-organisms will produce it. There are other means whereby art can produce acute inflammation, and the same may be to a limited degree true of nature, but the degree is so limited that such an occurrence is exceptional and rare, and can indeed be scarcely said to occur; the almost invariable rule being that micrococci are the producers of acute suppurative inflammations or phlegmons.

The literature of medicine has been of late strongly corroborative of this opinion, and of the views generally that were advanced in last year's report. Without going back on matters that were published before they were first made public in April 1880,<sup>1</sup> a short notice of what other observers have had to relate may be of value as showing that the tendency generally has been towards the adoption of the theory of inflammation and blood-poisoning emitted in the report. These were, briefly put:—"Acute and pyæmic abscesses always contain micrococci; may cause death by blood-poisoning, may cause sphacelus, or may be resisted by an unusually unsusceptible animal"

<sup>1</sup> Langenbeck's *Archiv*, vol. xxv.

(individual), and, "as a general rule, produce acute inflammation, ending in abscess. They are able, under suitable conditions, to give rise to blood-poisoning, to acute inflammation, and to suppuration."

The monographs that have appeared bearing on the subjects of blood-poisoning, inflammation, and suppuration, as well as the smaller contributions to pathology, all tend to establish the belief that these affections are mutually related, and have for their common cause micrococcus poisoning.

Among the minor papers may be noticed that of Dr. Dreschfield, who, in the Manchester Medical Society of 26th April 1881, is reported<sup>1</sup> as having shown micrococci from the pus of a woman who had died of septicæmia, with ulceration of the intestines and renal abscesses, and where the pus injected into a rabbit caused its death, the blood containing these organisms; also that of Dr. Stephen Mackenzie, who, at the Cambridge meeting of the British Medical Association, demonstrated<sup>2</sup> the micrococci in the blood-vessels in pyæmia, and in the metastatic abscesses and tissues of the lungs, heart, liver, kidneys, and thyroid body. Letzerich<sup>3</sup> describes the cultivation in gelatine, and gives photographs of the micrococci that he alleges to be the causes of diphtheria, infectious pneumonia, and influenza respectively, though I feel bound to state that the absence of a clear description of whence and how he obtained his results, as well as some discrepancies between his observations and those of other observers (e.g., Eberth, in the matter of the organism of typhoid fever), prevent his observations being conclusive or reliable.

Dr Otto Kahler<sup>4</sup> describes three cases,—one of gangrenous periostitis of the upper jaw with septicæmia; another of tubercular lung disease, purulent meningitis, embolic nephritis and septicæmia; and a third of septicæmia and gangrene of the finger, where, *during life*, embola were detected in the retina by the ophthalmoscope, and after death were found to be caused by colonies of micrococci; that also to all appearance gave rise to the other inflammatory, suppurative, gangrenous, and septic phenomena that were present.

Blaschko,<sup>5</sup> in a treatise that is all the more impartial that it was written for quite another purpose, viz., to ascertain the connection between stupor and cerebral changes in febrile diseases, gives ample confirmation of the important rôle assigned to micrococcus. He examined the brain and other parts of a number of women who had died of puerperal inflammations, gangrenous, or diphtheritic endometritis, and suppurative and septic peritonitis; of two men who had died of septic phlegmon of the arm; and of a series of rabbits and guinea-

<sup>1</sup> *Brit. Med. Jour.*, 1881, vol. i. p. 853.

<sup>2</sup> *Brit. Med. Jour.*, 1880, vol. ii. p. 386.

<sup>3</sup> "Untersuchungen über pathogene Schistomyceten," Kleb's *Archiv f. Exp. Path. u. Pharm.* 1880, Bd. xii. p. 351.

<sup>4</sup> "Ueber Septische Netzhautaffectionen," *Zeitschrift für Heilkunde*, 1880, Bd. i. p. 111.

<sup>5</sup> "Veränderungen bei fieberhaften Krankheiten," Virchow's *Archiv*, 1881, vol. lxxiii. p. 471.

pigs rendered septic by the injection of purulent and other liquids obtained from his human subjects, or by septicæmic blood or putrid liquids. He found the frequent presence in septic purulent peritonitis and pleuritis, myocarditis, &c., of micro-organisms, of which, by the way, he remarks—"This much, however, seems to me from my observations to be highly probable, that bacteria and bacilli, rod-shaped organisms, have nothing to do with the morbid process in question, but are concerned in decomposition; while, on the other hand, the *round forms are concerned in the production of the disease we know as septicæmia.*" Blaschko, however, so little saw the full bearing of the facts that he was narrating that they evidently sometimes puzzled him, as when he remarks with astonishment on finding micro-organisms "in the brain of an individual who had died of pneumonia!" The whole matter is, however, plain, and all his facts intelligible to those, and only to those, who recognise the full relationship between micrococci and the septic and inflammatory processes. *En parenthèse*, and on account of what will afterwards fall to be mentioned, I here note that Blaschko, "despite the use of the newer methods of Koch and the valuable dahlia-staining of Ehrlich, was not always able to demonstrate the presence of micro-organisms" in the blood in the animals that he had rendered septic. This is a point that will subsequently be referred to at length.

Schüller,<sup>1</sup> in connection with a case of acute suppurative osteomyelitis in a boy where he amputated and examined the affected limb, confirms the previous observations of Lücke, Klebs, Recklinghausen, and Eberth as to the dependence of this disease on micrococci. He found them in enormous colonies in the medulla, periosteum, and invaded tissues in the vicinity of the bone, and even in the canals and cells of the articular cartilage.

Many other papers, not written directly on the question at issue, also bear out the views concerning micrococci that were expressed in last report. As a specimen of these may be cited Eberth's *Researches on the Bacillus of Typhoid Fever*,<sup>2</sup> wherein it is easy to see how a disease dependent, like typhoid fever, on its own special micro-organism, and running its course according to the laws that it obeys, is yet often, owing to the lowering of the system and diminished resistance of the tissues that it produces, the means of enabling other micro-organisms, such as micrococcus, to enter the system, and produce septic and inflammatory changes that do not, properly speaking, belong to the disease itself, but are rather accidents accompanying or following it. Such complications of febrile diseases as endocarditis, suppurative synovitis, phlebitis, suppurative diseases of bone, lymphadenitis, and various phlegmons and abscesses, are evidently produced by the micrococci that enter the system under the ægis, as it were, of such febrile diseases, and form frequent complications and so-called sequelæ of them; although due as they are to different micro-organisms, the connection between them, though

<sup>1</sup> "Acute Infectiöse Osteomyelitis," *Centralblatt für Chirurgie*, 22d Oct. 1881.

<sup>2</sup> Virchow's *Archiv*, 1881, vol. lxxiii. p. 486.

common, is by no means a close one. They clearly depend on the property of the fever to behave in the same manner as the exposure to cold or other depressing influence that was urged in the former pages of this report as a favouring agency in the introduction of micrococci into the body. While this report was being written, a remarkable paper upon acute fibrous pneumonia by Friedländer has appeared,<sup>1</sup> demonstrating the invariable presence and the behaviour of micrococci in this affection, and referring to a similar demonstration of their connection with erysipelas by Koch, in a source that is at present inaccessible to me.

Having now cleared the field of preliminary matter, and left unanswered, I believe, no point of consequence in the adverse criticisms that have been advanced concerning such views as mine, I now proceed to the subject that more especially falls to be dealt with in this essay, viz., *blood-poisoning* (septicæmia and pyæmia), and the diseases intimately connected with it.

And here, again, it is well to preface some observations regarding a few preliminary matters, the decision and proper understanding of which are of no mean importance in enabling us to appreciate at their just value the work and observations that have been and may yet be made on this subject, and in attaining a uniform standard in deciding as to what amount of evidence is desirable in the various questions that compose it.

And, first, it is needful to decide as to what are the conditions that should have been fulfilled by observers in order that their observations should be acknowledged to possess any value or full value. It would be at once acknowledged as ridiculous if a writer on diatoms were to assert that the markings on some difficult form had no existence, while he admitted that he had used for their observation only ordinary reflected lamp-light and a half-inch objective, or if an anatomist were to deny the serration of the margin of epithelial cells at which he had looked only with a low magnifying power. Such observations would be recognised as being so inadequate that no one would bestow on them serious consideration; yet that is precisely what is being done without exposure and without recognition by the profession in the questions relating to bacteriology to a less glaring yet not less real extent. We know that the questions

<sup>1</sup> Virchow's *Archiv*. 1882, vol. lxxvii. p. 319.

to be decided are no easy ones, that the organisms under consideration are most difficult to recognise and demonstrate; we see that special means are required to become aware of even their existence under certain circumstances, and yet monographs are written and read and quoted and spoken of as of equal value and authority, in which one author has taken every means to render his observations secure, while another may have worked with utterly inadequate apparatus, or may not even have mentioned under what conditions his observations have been carried out. This is manifestly unfair to ourselves if we expect to arrive at the truth, and in future observers must be called upon to define the precise conditions under which they obtained their results, so that another observer may put himself under exactly the same conditions, and verify or refute their conclusions. If, at the same time, each writer were to mention exactly the conclusions his experience had led him to adopt regarding the means necessary to obtain reliable results in questions concerning micro-organisms, we would soon be in a position to say when and how precision was to be obtained. As a contribution to this matter, it seems necessary that it be plainly said that in the present state of our knowledge no observations can lay claim to the nearest possible approach to accuracy that have not been carried out with the oil-immersion objectives now in general use. Their superiority over the water immersion or dry lenses of equal power, for observation concerning micro-organisms, is simply enormous, and the experience of several years of work with lenses of all three sorts has left no doubt in my mind as to the necessity for bacteric observations being made by the oil-immersion systems. The same object may, under the latter, show large and beautiful colonies of micrococci, which under the other lenses may give *no sure evidence* of their existence. Micrococci being small and round, so as to resemble granules, are especially the organisms which, when occurring in the tissues, call imperatively for the employment of oil objectives. Of course the magnifying power of the lenses employed is no matter of indifference; in truth, the very reverse is true. It has been said that objectives of  $\frac{1}{10}$  inch and upwards are suitable for studying micro-organisms, and it is true enough that he who uses  $\frac{1}{10}$ -inch lenses will see more than he who uses  $\frac{1}{8}$  inch; but in the same way a  $\frac{1}{12}$  inch

will show what the  $\frac{1}{10}$  inch will not show, and so on, the greater the magnification the better being the results, until the limit where questions of illumination, &c., that come into play as the magnifying power increases, steps in to render very high lenses unsuitable for work. As illustrating the matter, I may say that I have some slides of micrococci growing in the tissues that are not visible with any lense lower than a  $\frac{1}{8}$ -inch oil-immersion objective, and that even with this, require special illumination and a somewhat high eye-piece (No. 4 Zeiss).

The use of aniline staining, combined with the employment of the Abbé's condenser, to obtain, as described by Koch,<sup>1</sup> a pure colour picture in the examination of micro-organisms among the tissues, is now generally recognised as a valuable help towards demonstrating their existence; and I can only add that, in my judgment, no observations are entirely satisfactory that have not been carried out, in all doubtful points, with the aid of the Abbé's condenser employed by Koch's method, as described in my former report. During the past year we have received a valuable addition to our methods of research in the introduction of the aniline *gentian-violet* staining by Weigert,<sup>2</sup> who has shown that gentian-violet in a watery solution of 1:100 stains the tissues, and at the same time the micro-organisms they contain so well, that subsequent steeping of the sections in alcohol always leaves the latter remarkably distinct and visible, which did not invariably happen with the older reagent methyl-violet. I have found it best, as I believe, to use a solution of gentian-violet of the strength of 2 grains to the ounce, to leave the sections of the tissues suspected to contain micro-organisms for twelve hours in this solution, then to transfer them for five minutes to water containing 5 drops of glacial acetic acid to 2 ounces of the water, then transferring them for two minutes into methylated spirits of wine, then for two minutes into absolute alcohol, then putting them into oil of cloves, and finally mounting in commercial Canada balsam, to which nothing has been added to dilute it, such as chloroform or benzole. This staining is of the greatest assistance, and seems to be the very

<sup>1</sup> *Wundinfektionskrankheiten*, 1878, p. 34.

<sup>2</sup> "Zur Technik der mikroskopischen Bakterien Untersuchungen," *Virchow's Archiv*, 1881, vol. lxxiv. p. 275.

perfection of a reagent for the purpose, as it never fails to stain all, even the finest organisms, and leaves them perfectly plain and distinct. For the examination of liquids it is best used like the methyl-violet in the way described in the former report.

By these means we have it in our power to render observations concerning micro-organisms really satisfactory and reliable; but any departure from these suffices, so far as I can judge, to throw them open to the charge of being inaccurate and unreliable, and I cannot bring myself to attach much weight to statements of those who are working with lower powers than the  $\frac{1}{8}$ , or, at the very least, the  $\frac{1}{2}$  oil-immersion objectives, and who do not at the same time employ the Abbé's condenser and the gentian-violet staining in the manner described by Koch and Weigert. I am sure that any one who has frequent opportunities of comparing the results obtained by other lenses and other methods, even those that seem to approach them in principle, will agree with me that this caution is but just and moderate. Hence, there are some writings concerning micrococci and their relations to inflammatory and septic processes that seem to me to be open to considerable suspicion. Uskoff, whose views were discussed at the beginning of this report, makes no mention of the means he used to verify his experiments, and this should of itself, in my opinion, until the deficiency be supplied, suffice to call for hesitation in drawing any conclusions regarding his observations, while, as already mentioned, his statement that in some cases the micro-organisms "could not be recognised with absolute certainty" seems, although I may possibly be wrong in judging thus, to show that Uskoff was not observing with all the aids of staining, Abbé, and the  $\frac{1}{8}$  oil. With these it may *sometimes* be a question as to whether organisms are present in some of the tissues, but in pus and liquids such as he was examining, so far as my experience goes, doubt on the subject should not exist. There is no organic granule, no fatty particle, that should in such liquids be capable of misleading the practised eye. A hasty glance may mislead, an eye unfamiliar with micrococci may be mistaken, but the clear round body of the micrococcus, even when isolated from any of its neighbours, as sharp-cut and clear-edged as the moon in the sky, is always to be discriminated from the organic granules that have been said in such liquids to



resemble them. Those who have stated this have themselves worked with means too imperfect to be allowed a voice in the decision of the matter. A little care and practice, which are not hard conditions to ask of those who venture to write on such subjects, will always suffice to discriminate isolated micro-organisms, and even micrococci the most difficult of all, in the organic liquids where they grow in animal bodies, and when they are grouped in pairs or large numbers, the matter is an easy one. In the tissues we can hope that higher and better oil-immersion lenses may offer us yet further information concerning these bodies, but in liquids they are so completely in our power as regards their recognition that such a statement as Uskoff's is strongly indicative of his having worked with less perfect means than are now to be everywhere readily obtained.

Another and a very valuable set of papers, published by Wolff shortly after Koch described the improved processes for observing micro-organisms, are open to the same objection. Wolff<sup>1</sup> examined the correctness of Koch's assertion that individual micro-organisms could be with certainty recognised. Even in regard to liquids such as pus and blood, Wolff concluded that Koch was wrong in making this statement. Now Koch might have been, I think, more cautious in his statements regarding *the tissues*; but in what concerns *the liquids* of the body, that can be spread out, stained, and observed in an almost ideally thin layer, Koch was certainly right, and Wolff appears to have been led to believe the contrary, because he used instead of the oil-immersion objectives the lenses F and immersion J (Zeiss). Now F is a  $\frac{1}{4}$  inch dry objective, and J is a  $\frac{1}{8}$  inch water immersion, and observations made with these lenses are not comparable in accuracy with those made under better optical conditions, so that I am compelled to deny to Wolff's observations all the weight that their industry and thoroughness would otherwise have obtained for them. It so happens that Wolff's observations run nearly parallel to my own in many matters, and in many points he denies that certainty exists where I believe it does. While, therefore, I concede to Wolff a great deal of merit and correctness, I cannot allow that he is always reliable, for it so happens that I have frequently been working with the very lenses he used, viz., the F Zeiss and the  $\frac{1}{8}$  water immersion, and can recognise the exactness of his statements, so far as these lenses go; indeed, I see that had I worked with these alone I should have arrived at the same conclusions as Wolff, but knowing, from habitual comparison, the superiority of the other lenses, I can discern that where

<sup>1</sup> "Zur Bakterienlehre bei accidentellen Wundkrankheiten," Virchow's *Archiv*, 1880, vol. lxxxi. p. 193, and also in the same volume, p. 385.

our observations are contradictory the difference is explained by the difference in the conditions under which we were observing.<sup>1</sup>

It has been suggested by skilled microscopists that granules such as belong to animal structures may be mistaken for micrococci, and among English authors Mr. George and Mrs. F. E. Hoggan,<sup>2</sup> in an exhaustive work on the transmutations of fat cells, have shown that certain stages of their life are attended with the production of innumerable microscopic granules, sometimes enclosed and at other times free in the tissues, which closely resemble colonies of micrococci, and have often been mistaken for them. Without hesitation, it may be admitted that this is quite true. Such cells exist, and are liable to give rise to error. But they are well known to most of those who occupy themselves with the subject. Koch has directed particular attention to them<sup>3</sup> as having been observed and drawn by Ehrlich in 1877, and under the name of *plasma cells*, has stated that they greedily absorb the same colours as micro-organisms, and are best discriminated from them by their losing their colour when exposed to treatment with weak solutions of carbonate of potash. The plasma cells I know well, and have often found that, *with insufficient powers*, they may cause perplexities out of which Koch's proposals do not help us. But when tinted and examined by Abbé and the  $\frac{1}{8}$  oil, they can be at once distinguished. The granules that seemed round are then seen to be of all manner of irregular forms and sizes, generally angular in shape but with rounded corners, often with their edges, if closely examined, fading off into the surrounding tissues, instead of being clearly differentiated as in micro-organisms, and they want the characteristic fish-roë grouping

<sup>1</sup> To the valuable observations of Braidwood and Vacher, that have appeared in the *Brit. Med. Jour.* for the present year (vol. i. p. 41 *et seq.*), in so far as they bear on the connection between micrococcus and inflammatory or septic diseases, I cannot admit any weight in the decision of the question. It is, indeed, there stated (p. 146) that "the septic influence of human lochia on rabbits is not connected with the presence in it of micrococcus organisms." But (at page 181) it is admitted that a magnifying power of 350 was the highest employed. Many micrococci are totally invisible, even when well prepared and stained, with powers greatly exceeding that magnification.

<sup>2</sup> "On the Development and Retrogression of the Fat Cell," *Jour. Roy. Micros. Soc.*, vol. ii. No. 4, June 1879.

<sup>3</sup> *Wundinfections Krankheiten*, p. 38.

which, besides their spherical form, distinguishes the micrococci. The danger of confounding them exists, it is true, but only to the unpractised and those using imperfect means; when properly viewed they are as unlike micrococci as a heap of unhewn stones is unlike a pile of cannon balls. There is no evidence in their essay that these observers used other means of observation than a magnifying power of 800 diameters, and I have shown that this does not fulfil the needful optical conditions.

It has been decidedly stated, and as flatly contradicted, that the *healthy tissues contain no micro-organisms*. It is somewhat difficult to understand the investigations that have been made in the matter, for all who have attempted to answer the question by direct observation have been unanimous in stating that the tissues are free from germs, while most of those who have approached it from the experimental side have given the reverse reply.

The well-known experiments of Billroth<sup>1</sup> of plunging masses of flesh removed from freshly killed animals into molten paraffin, and there abandoning them to their fate, showed micro-organisms growing in and decomposing them. The experiments of Chiene and Ewart<sup>2</sup> of removing similar portions under Lister's spray and antiseptic precautions, gave the opposite result; but I am informed by the former of these observers that a subsequent set of experiments, performed under his directions, showed micro-organisms in the tissues.<sup>3</sup> Again Arndt,<sup>4</sup> whose experiments were similar to those of Chiene and Ewart, found that organisms always developed in the portions of tissue he removed and submitted to experiment, so that from this side the reply has been adverse to the healthy tissues of animals being free from micro-organisms.

But it may be that the tissues of animals in captivity and under like circumstances with those employed for experiment, are not fair representatives of healthy human tissues, but may resemble diseased structures in containing organisms. Again, the observations of Mikulicz<sup>5</sup> have shown that in such experiments the protecting influence of the spray is by no means great, a fact that I can personally

<sup>1</sup> *Coccobacteria septica*, 1874, p. 59.

<sup>2</sup> *Jour. of Anat. and Phys.*, vol. xii. p. 448,

<sup>3</sup> Since this was written the manuscript of this unpublished investigation has kindly been lent me for perusal by Dr. Fras. M. Caird of Edinburgh, who carried out the series of investigations.

<sup>4</sup> "Ueber die Entstehung von Kokken und Bakterien in Organischen Substanzen," *Virchow's Archiv*, 1880, vol. lxxxii. p. 119.

<sup>5</sup> "Zur Sprayfrage," *Arch. f. Clin. Chir.*, vol. xxv. p. 707.

corroborate, as a careful series of experiments hitherto unpublished concerning the protective influence of the spray carried out in the Aberdeen Infirmary by Messrs. Davidson and Prain, yielded results somewhat similar to those of Mikulicz in experiments such as have been made by the above mentioned investigators.

If the spray be not absolutely reliable these experiments lose their value, since the pieces of tissue are then open to be infected while passing through the spray,<sup>1</sup> a thing hitherto assumed to be impossible.

In the present state of our knowledge regarding the capacity of germs to withstand heat and spray, a satisfactory experimental test as to whether the tissues contain them seems to be an impossibility.<sup>2</sup>

Hence we are driven back upon direct observation. And here our answer to the inquiry that concerns us is almost if not entirely unanimous. No one has ever seen a micro-organism in a healthy tissue, or in healthy blood or urine. It is now many years since Mr. Lister showed that freshly passed urine contained no organisms, and this has not only met with no denial, but on the contrary has been found axiomatic in all experiments on germs. Personally I have performed many experiments concerning micro-organisms that depended for success on the sterility of fresh urine, and have never once been deceived. There is standing before me as I write a flask that, after being disinfected on the 29th April 1880, by two hours' exposure in a hot chamber to a heat of 340° Fahr., was filled on the 23rd of March 1881 with urine, by being carefully uncorked and without exposure of its contents, having urine passed into it from the disinfected meatus urinarius. The urine in that flask has been used for many an experiment, and has been tested micro-

<sup>1</sup> An objection that likewise applies to the experiments of Mott and Horaley (published in the *Journal of Physiology* for January 1882, vol. iii. p. 188) that seem to confirm those of Billroth. Tyndall's strange experience with hay germs, that render it intelligible why one locality may be so much more infected than another, suggest a possible explanation of the discordant results obtained by investigators.

<sup>2</sup> Nature is constantly performing for us a series of immaculate experiments. Every long standing effusion of serum and blood into serous cavity, joint, cyst, or hæmatoma, is an experiment under conditions absolutely perfect, highly sensitive liquids being exposed for long periods, at proper temperatures, to contact with the human tissues. The invariable reply we get from these is, that our bodies contain no micro-organisms.

scopically on more than six occasions, and yet what remains of it is, as it has always been, free from any organism and pure, save for a few crystals of uric acid.

Max Wolff, despite the dubiety in which his methods left him regarding other parts of his inquiry, says:<sup>1</sup>—"Contrary to the assumption of many authors, I must completely agree with the statements of Klebs, Sanderson, Pasteur, and Koch, that *bacteria or their germs do not occur in the blood of healthy men and animals.*" Wolff here uses the word "bacteria" as signifying "micro-organisms." Tyndall<sup>2</sup> says:—"The liquids of the healthy animal body are also sealed from external contamination. Pure blood, for example, drawn with due precautions from the veins, will never ferment or putrefy in contact with pure air."

I performed the following experiment with the navel strings of newly-born children. In three children immediately after birth, the navel string was surrounded by two ligatures half an inch apart at the end next the foetus, and these were tightly tied. A third ligature was tied close to the navel of the child. The cord was divided close to this last ligature between it and the two others. Two ligatures, also half an inch apart, were then placed round the placental end of the cord, and tied, and the cord was divided between them and the placenta. Thus the cord was separated full of blood, each end being doubly secured. The cord was carried home, washed by a five per cent. solution of carbolic acid, and the plug of cotton wool removed from a Lister's flask, that had been heated for two hours in a hot chamber at a temperature of 360° Fahr. A Lister's spray was playing over the mouth of the flask while the plug was being removed, and the plug and the neck had both been well soaked in the five per cent. carbolic lotion. One end of the cord carrying its double ligature was brought near the mouth of the flask, and cut off with a scissors that had been washed in the carbolic lotion. The open end of the cord was then put for half an inch into the flask, and the cord was suspended by its other end vertically above the flask. The blood thus escaped into the flask. The flask was closed by a cork of cotton wool wetted with the carbolic lotion, and set aside. In the three flasks so treated no decomposition set in; a little serum was in a few

<sup>1</sup> Virchow's *Archiv*, 1880, vol. lxxxi. p. 197.

<sup>2</sup> *Floating Matter of the Air*, 1881, p. 243, footnotes.

days squeezed out of the clot, but it never rotted or contained any micro-organisms, and the clot remained fresh and unchanged until six months had elapsed, when it was examined, and found free from any decomposition or micro-organisms. The serum of the blood was used for many experiments concerning the culture of micro-organisms upon glass slides, and was over and over again examined for micro-organisms without one being ever found.

In a foetus of the fourth month of utero-gestation, that was born dead and showing the usual signs of intra-uterine maceration, and that, after passing spontaneously through the passages and lying one and a half hours in the bed, was detached and kept for five and a half hours wrapped up in a towel, an examination of many slides of liquid taken from every part of the alimentary canal from the mouth to the anus, and from the nostrils, middle ears, trachea, gall bladder, pelves of kidneys, and urinary bladder, revealed the complete absence of micro-organisms from the whole of these cavities, while its cutaneous surface showed a few bacilli, evidently derived from the maternal passages. On one occasion I had an opportunity of examining the liquor amnii, and found it free from any organisms. Thus the dead foetus, a most favourable soil for the development of micro-organisms, in going on to intra-uterine maceration without any such bodies appearing, furnishes the most pointed evidence as to the healthy ovum being free from any germs, and to the tissues around it, from which after its death it could presumably with great readiness have received them, being likewise free.<sup>1</sup>

I have not counted the number of times that I have carefully examined slides of the blood and urine of healthy individuals, but I am sure I am very far within the mark in stating them as fifty; yet I have never once seen an organism under such circumstances.

It is therefore, it seems to me, permissible to formulate what we know, in words like those of Wolff, viz., *micro-organisms do not exist in the blood and tissues of healthy men.*

<sup>1</sup> Mr. Tyndall remarks (*Floating Matter of the Air*, 1881, p. 36),—"Dr. Burdon Sanderson draws attention to the important observation of Brauell, which shows that the contagium of a pregnant animal, suffering from splenic fever, is not found in the blood of the foetus, the placental apparatus acting as a filter, and holding back the infective particles."

But if they do not exist within, they are present in enormous numbers without. They not only rest, but develop on the surfaces of the human body. Examination of the skin on regions much subjected to frictions and ablutions, as the hands, fronts of the thighs, arms, and fore-arms, shows micro-organisms so sparingly, that epidermis removed from thence by scraping may often exhibit few or none. But on individuals who, from any cause, do not practise frequent ablutions, even on these regions they are usually found, while in not even the most cleanly person does the interdigital scurf of the toes fail to show abundance of both micrococci and bacteria. The scurf of the scalp contains mostly fungi and fungus spores, and is by no means rich in cocci or bacteria, while the secretion of the umbilicus shows both these forms to be present in abundance.

Micrococci are numerous in the cerumen of the ears, and in the mucus on the lips, and there exceed in number the other forms of organisms.

In the axilla, on the other hand, where micro-organisms are in great plenty, the bacteria preponderate, and in the secretion from the skin of the nose, that is mostly sebaceous, obtained by gently compressing its tip, almost the only forms met with are the bacteria, which are there, however, innumerable.

Sebum expressed from scrotal or perineal follicles shows multitudinous bacteria, slender rods, and micrococci; the anal region shows thick and slender bacilli, bacteria, and micrococci; while the fæces and interior of the bowels present the appearance of being entirely composed of infinite colonies of all varieties,—micrococci, large round and oval spores of fungi, slender bacilli, thick bacilli, bacteria of various kinds and sizes, and a few spirilla. All these organisms present, in the chains, groups, and forms in which they occur, the plainest evidence that they have been growing and developing, not simply reposing in a quiescent state.

If we assume the contents of the intestines to amount to twelve fluid ounces, and grant that each minim contains 20,000 micro-organisms, an estimate vastly less than the truth, we find that we each carry in our intestines over 109,000,000 of micro-organisms, without reckoning those that exist on the skin and in its gland openings, which must amount to several millions

more, as could readily be shown. But all such estimates are inaccurate and unscientific, and are of no use save to show that we need not even call into account the solids, fluids, and gases around us in our search for sources of infection, when we possess on our own frames so abundant a supply. In relation to surgical questions, it is of use to know the extent and distribution of these germs on our persons, if our processes of disinfection are to be conducted aright. In operative procedures on the axilla and scrotum, for example, we ought to know that all our preliminary washings and disinfection are impotent to exterminate the micro-organisms that exist in the openings of the glands to a depth of a quarter of an inch and even more. None of the proceedings in use in antiseptic surgery is of any avail to destroy them; they will continue to grow and reach the surface, and unless we maintain there a storehouse of some disinfectant material frequently renewed that will suffice to saturate all discharges and convert them into aseptic fluids, we shall assuredly find the organisms growing richly under our dressings. And this is what really occurs; in the former report I showed (page 373) that antiseptic dressings in such situations usually contain a variety of organisms. Extended experience has convinced me of the impossibility of maintaining such wounds free from organisms, and the need for the constant renewal of disinfectants, for keeping the wounds *antiseptic* in fact, as well as *aseptic*. Iodoform and salicylic acid, which are treasure-houses of disinfection, are more needful in those regions than on the arms, legs, and hands. But even in these purer territories over-confidence is perilous, and where we are dealing with skins not regularly cleaned, as is the case, for instance, to a large extent in hospital practice, we have to add to our habitual precautions, and attend both to preliminary saturation of the epidermis with penetrating disinfectants, and to the subsequent maintenance of stores of disinfectants on the surfaces where the germs may develop and appear. For a considerable time back I have found it advantageous in any operation that involved serious danger, and that required to be successful at all hazards (operations of complaisance, such as osteotomies and joint operations), to dress the part for days before operation in a regular Lister's dressing, renewing it daily and saturating the skin with carbolic water,



besides washing the part with turpentine immediately before making the first incision. I do not think I am wrong in saying that we are too easily satisfied with our cutaneous disinfection, and that the chief source of micro-organisms in wounds is from the skin, rather than from the air or failures in our antiseptic procedures.

The mention of the organisms that exist on the surfaces of the human body naturally suggests the inquiry, are they innocent or noxious? and it is unfortunate that we are, as yet, unprovided with direct proof concerning this. No one has cultivated them and directly shown that they produce disease in man. This gap is indeed a difficult one to fill up. We know that each genus of animals has its own diseases, and that these, as a rule, do not attack other genera of animals. The bacteric plagues of cattle are not those of man, and it is likewise in the highest degree probable that the septicæmia of man is not identical with that of cattle and other animals. Koch has shown<sup>1</sup> that the septicæmia of mice depends upon a minute bacillus, and that the pyæmia and septicæmia of rabbits are due to micrococci.<sup>2</sup> But no one has asserted that bacilli are to any appreciable extent connected with septicæmia in man; indeed, the numerous investigations into the human blood and tissues in this disease have presented a singular uniformity in demonstrating the absence of bacilli. The observations concerning the diseases of different animals, such as splenic fever, pig typhoid, and fowl cholera, and those of man, such as malaria, typhoid fever, and typhus fever, now numerous enough to furnish a considerable body of evidence, are all contradictory of the idea that human diseases are identical with those of animals; and although one genus can sometimes tolerate the importation of maladies from another genus, yet the very cases where we know of this are also corroborative of the idea that there is a strong distinction between genera in this respect. Consequently we cannot obtain a direct experimental reply to the inquiry unless we are in a position to experiment on man himself. Nothing is easier than to cultivate human organisms by allowing a portion of skin to putrefy in pure water, and from them to infect mice with the

<sup>1</sup> *Wundinfektionskrankheiten*, p. 40.

<sup>2</sup> *Ibid.*, pp. 54, 59.

septicæmia described by Koch. It is the easiest way of repeating his observations, and from the mouse it is a simple matter to obtain its septicæmic bacillus pure from other organisms. But how are we to isolate the organism that causes septicæmia in man? Not by ordinary cultivation and injection of the various organisms found on his body; for though it may be among these, we cannot distinguish it from the others. In injections into animals of liquids wherein various organisms from the human body have been promiscuously cultivated, we cannot be certain that they will be receptive of the organisms that prove injurious to man, and we fail to obtain the evidence we want. The septic germ of the *genus homo*, in the full virulence of its culture in the human body, may indeed be capable of being grafted on to many animals; but the same organism is likely to be ingrafted with difficulty, if at all, when taken out of the comparatively unsuitable fluids where we have been rearing it. Hence it need excite no surprise if we fail in experiments in this direction, nay, it is even no wonderful matter that we find, although the fluids of human septicæmia produce disease in animals when at once injected, that they nevertheless, when cultivated in various fluids, become less and less virulent and eventually innocuous to animals.

If there be one point on which we have received light in recent years, it is this, that the part played by cultivation in determining the activity and power of micro-organisms is of the very highest importance. We find that Buchner,<sup>1</sup> growing the innocent hay-bacillus in suitable media, converted it into the bacillus of anthrax or splenic fever; and, equally wonderful, by suitable cultivation converted the anthrax bacillus into the harmless hay organism. Nor is this strange experiment in evolution without support from other sources. Grawitz<sup>2</sup> changed the ordinary fungi of common mould into virulently poisonous agents by means of cultivation. Pasteur<sup>3</sup> and Toussaint<sup>4</sup> have altered the qualities of anthrax and fowl cholera by similar means, and my own observations that micrococci preserve their virulence and activity, only under certain conditions, points in a like direction.

<sup>1</sup> *Milzbrandcontagium aus den Heupilzen*, 1880, pp. 409, 410.

<sup>2</sup> "Schimmelvegetationen im thierischen Organismus," *Virchow's Archiv*, 1880, vol. lxxxi. p. 355

<sup>3</sup> "Sur la non-recidive de l'affection charbonneuse," *Compt. Rend.*, vol. xc.

<sup>4</sup> *Compt. Rend. de l'Acad.*, vol. xci. Nos. 2 and 5; also Chauveau. The same, No. 16.

Clinical experience corroborates the result of these researches, and points to the frequent development of common micro-organisms into forms capable of causing disease. In contrast with such maladies as variola and typhus, that are incapable of being generated by forces ordinarily in operation, we have pyæmia and erysipelas, which, if they be due to bacteria at all, must be produced by those among which we are constantly living. We see these diseases everywhere, arising under usual circumstances, occurring under conditions that preclude the supposition of their being allied to some rare form of organism; we can even, by want of cleanliness, overcrowding, and bad hygienic conditions, generate them at our pleasure, so to speak, out of agencies that are always with us. Malaria furnishes another instance of the same fact. It cannot be that the bacillus of malaria is peculiar to certain situations, for we find it wherever conditions exist favouring its development out of the germs present in the soil, and it is even known to reappear in localities whence it seemed long to have been banished, on an upturning of the soil presenting anew the conditions under which it of old arose. Clinical experience is indeed apt to mislead us, but *some* weight must be allowed to it, and what we see is capable of explanation only on the supposition that favouring circumstances can evolve many of our bacteric maladies out of the micro-organisms that lie at our doors.<sup>1</sup> Unless we are prepared to deny the truth of almost all the observations of the last few years, we are driven to expect, in the common organisms of the world, the source of many diseases that we believe to be more or less of the nature of blood-poisons.

I would wish here to enter a *caveat* concerning the misleading ideas suggested by the word "blood-poisoning" and its equivalents, that have given a false direction to our thoughts and observations regarding bacteric diseases. The prevalent idea clearly suggested by the name is, that the micro-organisms that cause diseases *grow and multiply in the blood*, and there find their chief development, so that in observing and studying, our

<sup>1</sup> The experiments of Buchner and Grawitz are pregnant with explanations of how our epidemic maladies at first arose, viz., from ordinary germs cultivated under circumstances that may perhaps never be repeated, and harmonise well with what we know of their origin, alterations, and occasional disappearances.

chief attention is directed to this fluid, and perhaps concentrated on it alone. Can this be shown to be reasonable?

It is true that in several diseases that have excited much attention, of which splenic fever may be taken as the type, the blood has seemed to be the favoured residence of the diseased organisms, and their introduction into the blood, their multiplication there, their demonstration in it, and the power of infecting animals through it, have so occupied our thoughts that it has never occurred to any one to ask, Is the blood the chief site of growth of the organisms? But if we question nature we are apparently answered, especially in other and parallel diseases, in a somewhat different direction. The lessons we receive from smallpox, measles, and scarlet fever, for example, force us to the conclusion that the blood is not the chief site of the disease. We see that the moment of infection is followed by a period of quiescence, during which something is being prepared. There then ensues a feverish condition, attended with pains and signs of some internal lesions, such as the backache of variola, the cough of measles, or the scarlatinal throat, followed by the appearance of an eruption on the skin, at first of minute points, gradually becoming larger, appearing first on the upper and descending then to the lower parts of the body, often sparing the extremities of the limbs, and advancing in harmony with the fever until it and they reach their acme at the same moment. How can we explain all this if the blood be the part chiefly affected?

But if, applying what the study of the life of these bodies has taught us, we remember their tendency to multiply into groups and masses, and assume that they are no sooner introduced into the body or the blood than each isolated organism tends to develop into a group or mass, we perceive that this necessarily leads to their being detained by the capillaries in the tissues, where they will continue to increase into larger foci, whence individuals are being constantly washed away by the blood to form fresh masses, that in their turn are detained somewhere else in the tissues, and so on till the whole of the suitable solids of the body are studded with foci, from which the blood is supplied with individuals that are as constantly being filtered out in virtue of their own increasing size. This process alone pre-

sents to us a reasonable picture of the events that occur during the stage of incubation, with its slight malaise and slight rise of temperature. This stage, we must believe, terminates when the temperature is sufficiently disturbed throughout the body to evidence itself by the rigor and shivering that initiate the visible disease, for rigors and shivering are but a sign of a temperature elevated to a certain degree. In the parts of the body where the temperature is highest, growth will be most rapid, and hence the cough or sore throat, symptomatic of changes due to internal foci, precede the outward skin eruption. But the cutaneous foci have their turn, although the lesser temperature of the outward parts retards their full development; and hence, in due time, we see the cutaneous eruptions appear as the foci there become large enough to produce in the skin reaction and irritation; and they attain maturity earliest where the temperature is highest, as on the head and chest, often sparing the hands, feet, and extreme parts of the limbs, where the temperature is too low for their full luxuriance.

If we assume that it is in the tissues that the disease germ multiplies, and that the blood is not its main theatre, we can explain the course of these diseases; if we deny this, we fail to do so. I have often watched with interest the growth of these spreading foci during the eruptive stage of zymotic diseases, and have become convinced that some such law as that which I have developed above regulates the phenomena of all bacteric morbid processes.

Every bacteric disease illustrates the law. The intestinal glands and spleen of typhoid, the skin eruption and local depôts of typhus, and the splenitis and hepatitis of malarial fever, are but indications of the universality of the tissue colonisation that is a more important factor than the alteration of the blood; while even in splenic fever the enormous development of the bacilli in the lungs and enlarged spleen as compared with other parts shows that this disease also, if closely looked into, would be found to illustrate the same principle. It is possible that the law of tissue colonisation might lead to an explanation of the confusing results of investigations concerning the connection between relapsing fever and the spirillum of Obermeyer, hitherto sought for only in the blood.

If these views possess any truth, it will not be without a bearing on human septicæmia. It has hitherto been the fact that those who approached the study of this subject always did so possessed with the prejudice suggested by the name itself, viz., that the cause of septicæmia was to be sought for and studied in the blood. One observer after another has examined, analysed, and experimented on the blood, and estimated his results entirely from this point of view. Chemical changes have been sought for, micro-organisms have been searched for and found or missed, experiments have been made with varying results, all under the dominant idea that the peccant material was to be met with there, if at all, and the result has been that in regard to no other subject are contradictory opinions more rife.

Yet if what I have advanced be true, if our mental vision has been obscured by the term blood-poisoning, and the assumption that we were dealing with a condition analogous to what we imagine, perhaps erroneously, to be present in anthrax, we must be prepared in future to abandon these views, and to look for further progress in the direction of regarding blood-poisoning not as a poisoning of the blood *per se*, but as a disease existing in the tissues, from which the blood is but secondarily affected to a greater or less extent. We must abandon the idea that the blood is the one thing needful to be examined and reasoned upon, and look on that fluid as merely a vehicle that may generalise in the body what without its aid would be but a local process, through its tendency to clear itself of the noxious organisms it may carry by a process of filtration in the capillaries of the tissues.

We must look to the tissues as the sources of our septicæmia and pyæmia, must scrutinise narrowly the morbid states in which we find them, estimate their casual connection with micro-organisms, measure the intensity of the general disturbances that may occur by the intensity and extent of the local damage, and admit that general disturbances are merely due to the fluids or ptomaines that the blood carries away with it as the result of its passage through the diseased foci. We shall have, it is true, still to examine the blood and observe what it carries with it, but with the distinct recollection that what we find in it is but a portion of the disease, not the whole malady.

We shall be compelled to abandon the idea of reckoning the number of micro-organisms in the blood and drawing from thence conclusions as to the influence they may have in accounting for the symptoms and conditions that we observe, and realise that their presence is but the expression of a process that is carrying on its development elsewhere, although a certain reasonable connection between the two may be presumed to exist.

The conviction that human septicæmia and pyæmia can be investigated with accuracy only from the point of view of their being primarily tissue diseases and secondarily blood diseases, has not been adopted on mere theoretical grounds, but is the outcome of prolonged observation. Although it is stated in a preliminary manner here, as a guide to the understanding of what must follow, it is not to be inferred that the study bestowed on these diseases was consciously influenced by it or any other preconceived idea whatever. On the contrary, I, like others, entered on my observations with the full conviction that the morbid changes in the blood were the chief, and all other changes secondary elements in the process, expecting to clear up everything by a study of the blood, and, like them, was disappointed at the results thus obtained. It was only little by little, as experiments on animals revealed the great work that goes on in the tissues as compared with the blood, that the truth forced itself upon me. Time after time, as detailed in the former report, has the inoculation of micrococcus into the tissue been seen to be followed by its immense growth and invasion there, and the outspreading and invading colonies that pushed in every direction and in dense hosts from the point of injection, as revealed by the processes of staining and observation already described, produced upon my mind a powerful impression of the might and activity of the processes carried on in the solids of the body as contrasted with the comparatively small evidence of the part played by the blood. It seemed indeed that the blood was but a carrier of the disease, nothing more. Here and there the blood showed a micro-organism, or one or two, or a short chain, but nowhere were there the compact hosts observed in the tissues, and it became evident that it was not in the blood that the theatre of the disease was to be sought. The blood could only claim to be the bearer of fugitive and stray members

of the organisms that were at work, and of the poisonous ptomaines that were the results of the action going on in the tissues. Indeed, it almost seemed as if the liquids of the body were not by any means suitable for the active growth of the micrococcus; for even in the process of formation of abscess, which was the usual result of the central breaking down of the infiltration of the tissues by the micro-organism, it was observable that, though its growth and development continued after the solid part had been changed into a liquid, the activity was diminished, and as the pus collected in quantity by liquid being shed into the cavity from the structures around it, the proportion of the micrococcus to the liquid became less and less, until at length, when the matter had increased to a considerable extent, there might often, from the scanty numbers of micrococci in a given proportion of the pus, readily occur to an observer who had not followed the process throughout, doubts as to whether they had any casual connection with pus-formation.

In truth, however, even as concerns blood and pus, the smallness of the bodies they contain, cells and micro-organisms, is so little realised, that we are apt to underestimate the full bearing of what we really do see. It is not easy to form an estimate of how much of these fluids we can examine at one time. A considerable familiarity with measurements by the hæmacytometer has led me to the belief, which I think is not far from the truth, that in preparing microscopic slides for examination we cannot on each submit more than 0·0005 of a cubic millimetre of fluid to a thorough exploration. Now, if this be true, and if we were to find only one micrococcus out of ten slides of blood, we would still have the large number of 177,400,000 micrococci in the blood of a person weighing 140 lbs. (63·0 kilogrammes) taking the blood<sup>1</sup> at 0·071 of the whole body weight. And in examining pus, if we found only ten micrococci on each slide, a quantity vastly less than it has ever fallen to my lot to observe, we would have 1,133,980,000 of cocci in a couple of ounces of pus. These numbers, however, are of value only as reminding us of the minute objects we are dealing with, and the myriads in which they may be present, although they seem few from the manner in which we have to observe them. When, however, we regard

<sup>1</sup> Hermann's *Handbuch der Physiologie*, 1880, vol. iv. part 1, p. 137.



septicæmia in its true light as a tissue disease, we shall cease to attach an exaggerated importance to the detection and enumeration of micro-organisms in the blood, for it is obvious that the gravity of the symptoms and the danger to the individual are great, not in proportion to the number of the organisms present in the blood, but to the numbers that are growing in the tissues at the local centres of growth, and the quantity of soluble ptomaines they are pouring into the circulation. It is even conceivable that were the tissue colonies composed of organisms that tend greatly to cohere and not break up into detached individuals, there might exist a septicæmia that would prove deadly from the amount and virulence of the ptomaines thrown into the circulation, while the appearance of micro-organisms in the blood would be scarcely if at all detectable. As a matter of fact, however, as will afterwards appear, although it is most true that micrococcus tends to grow in masses, not to disperse into individuals, yet the quantity of the micro-organisms detectable in the blood does usually bear a rough proportion to the severity and extent of the disease.

*(To be continued.)*

THE ACTION OF SALINE CATHARTICS. BY MATTHEW  
HAY, M.D., *Demonstrator of Practical Materia Medica in  
the University of Edinburgh.*

(Continued from page 440.)

SERIES OF EXPERIMENTS, D.

The effect of the salt on the alimentary canal, and the absorption of the salt from the canal, as ascertained by killing the animals at stated intervals after its administration by the mouth, and measuring the fluid in the canal, and estimating the quantity of salt present.

The methods by which I have arrived at the conclusion that a purgative salt excites secretion within the alimentary canal are those of the two preceding series of experiments. The former of these methods is not without objection, as I have more than once admitted, although I have attempted at some length to define in how far the conditions of the method modify the action of the salt. And it may be urged against the latter method that the relative increase of the blood-corpuscles is no direct or positive proof of the serum being diminished; we merely infer that there is diminution of the latter, because we do not believe it possible that the total number of the corpuscles in the blood can be as suddenly increased as the results of the B. Series of Experiments would otherwise imply.

I have now to describe a method, which, from its extreme simplicity and the absolute normality of its conditions, is free from every objection. It suggested itself to me in the course of my experiments on the exposed intestines of the cat. In these it will be remembered that the animal received no food of any kind on the day of the experiment, nor for more than twenty hours previous to the operation, and that the diet was uniform in kind and quantity for at least a week previously; care also was taken to observe that the faeces had been for some days of their natural firm consistence. Under these conditions I had frequent opportunity of observing the state of the alimentary canal of the various cats operated upon. And I always found that on opening the abdomen the small intestine in its whole

length was completely collapsed and appeared perfectly empty; and, if I attempted to press out the contents of the intestine through the incisions made for washing it, never more than one or two drops of mucus escaped, mixed now and then with a small quantity of brownish shreds of undigested food, and frequently with fragments of tape-worms. Or, if I laid open the whole length of the alimentary canal, as I did on more than one occasion when the animal was killed intentionally or accidentally by an overdose of chloroform, the canal was practically empty excepting the large intestine, which always contained more or less of brown firm fæces. The total quantity of mucous fluid found in the stomach and small intestine never exceeded 5 c.c., and was in most cases practically *nil*. The contents of the colon were always firm, and quite as hard as evacuated fæces. In all my experiments, where the intestine was ligatured and injected, and the animal killed after a certain number of hours, the condition of the remainder of the canal was always carefully observed and noted after death. In looking over these protocols I found that the stomach usually contained from 1 to 2 c.c. of yellowish fluid of an alkaline reaction, or very rarely acid. Only in one instance did the quantity of the fluid attain 5 c.c. From the whole length of the small intestine, excluding of course the part operated on, one or two drops of mucus were generally obtained, and on no occasion more than half a cubic centimetre. The contents of the colon were as described. I therefore felt perfectly warranted in concluding that the alimentary canal of a healthy cat contains virtually no fluid twenty to twenty-four hours after a meal. Now, if a saline purgative be administered to a cat in this condition, it is evident that by killing the animal at stated intervals afterwards and ascertaining the amount of fluid in the canal, it will be possible to obtain a fairly accurate estimate of the quantity of secretion excited by the salt, if secretion does actually take place. There is no disturbing element in the form of any vivisection operation. This method further promised, by the opportunity it gave me of analysing the quantity of the salt recoverable from the alimentary canal, the means of ascertaining with what rapidity the salt was absorbed, and whether or not it underwent the remarkable variations in quantity—first being

absorbed and afterwards excreted—which Headland mentions, and to which I have fully alluded.<sup>1</sup>

The details of the method employed in each of my experiments hardly require description after what I have just said. The cat, for cats were always used, was fed regularly on a stated quantity of boiled flesh for at least a week previously, the condition of the *fæces* being observed, and the animal rejected if the excrement was in the smallest degree more soft than normal. The last meal was given on the day preceding, and not later than twenty hours before, the experiment. During this interval water, as well as food, was withheld from the animal. The experiment began with the injection of the solution of the purgative salt into the stomach; and this was made through a vulcanised tube passed down the *oesophagus*, so that no part of the solution was lost. As the cat was extremely apt to vomit shortly afterwards, its attention had to be distracted for half an hour or longer by frequent stroking. Even in spite of this it often vomited, and delayed for a few days the experiment. At the expiry of the desired interval, if purging had not previously taken place, which was not unfrequently another source of delay, the cat was killed as in former experiments. As the fluid *fæces* were extremely apt to escape from the anus during the struggles which preceded death, special care had to be taken that none was lost. This was best effected by transfixing and ligaturing the anus immediately after stunning the cat. In some cases, where the amount of fluid in the various portions of the alimentary canal was to be ascertained unaffected by the changes produced by the convulsions of the whole muscular system which preceded death, the abdomen was at once opened after tying the anus and before the animal died, but whilst it was unconscious, and ligatures were placed round the canal at various points. The fluid present in the canal was measured along with what solids it might contain. These in the fluid from the small intestine were never more than fragments of tape-worms; in the large intestine, a quantity of solid *fæces*. The contents of the large intestine were evaporated to dryness over the water bath; and, by adding to the weight of the residue the proportion of water which the ordinary unevac-

<sup>1</sup> P. 246.

uated fæces contained, I had the means of ascertaining with considerable exactness to what extent the quantity of the fluid had been increased by the normal contents of the colon. The percentage of water in the normal contents of the colon I estimated in several other cats, and found it to be fairly constant. These estimations I now give:—

Total Weight of the Contents of the Colon.	Weight after drying at 100° C.	Percentage of Water.	Quantity of $\text{H}_2\text{SO}_4$ , as $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ .
grms.			grms.
4·34	1·516	65	0·016
4·34	1·448	67	...
10·27	3·321	68	...
3·72	1·338	64	...
3·91	1·408	64	...
9·541	3·564	66	0·063
12·804	3·100	76	0·101

The average percentage of water in these seven different quantities of unevacuated fæces is 67·1 per cent. As this proportion of water in normal fæces is not absolutely definite and invariable, and, as the average obtained may have been a little too high from the exceptionally large percentage in the last quantity of fæces, I have for convenience considered the usual percentage to be 66·6, which allows of the weight of the faecal matter in the colon being obtained from the evaporated residue by simply multiplying the weight of the latter by three. Where the purgative salt is mixed with the fæces, its weight must, of course, be estimated and deducted from the total residue before trebling. The weight of the contents of the colon were thus ascertained, and, by subtracting it from the total quantity of fluids and solids in the alimentary canal, it was easy to gain a tolerably correct notion of the quantity of the secretion excited by the administered salt.

In order to estimate the quantity of the salt in the alimentary canal, the canal from the cardiac orifice of the stomach to the lower end of the rectum was removed from the abdomen, slit open in its whole length, and infused repeatedly—generally eight to ten times during three or four days—in distilled water. The various infusions were mixed and evaporated to dryness; and the residue, added to that previously obtained from the

contents of the canal, was sprinkled, unless where the sodium was estimated when lime was used, with a small quantity of dried carbonate of soda, and burned. The ash was dissolved in hydrochloric acid, and the sulphuric acid estimated by the usual method.

From a number of experiments given in an earlier part of this paper,<sup>1</sup> I concluded that 5 grammes of crystalline sulphate of soda was a dose sufficient to purge with certainty a cat of average weight. The cats selected were, as much as possible, of similar weight, full-grown, and well-conditioned.

The first three experiments were made principally with the intention of observing the effect of different degrees of concentration of the salt on the amount of the fluid within the canal.

*Experiment LIX.*—Cat, male, weighing 3·28 kilogrammes. Injected *per os* into the stomach 5 grammes of sulphate of soda dissolved in 100 c.c. of water, or a 5 per cent. solution. Killed exactly *one hour* afterwards.

*AUTOPSY.*—The stomach contained 1·5 c.c. of alkaline, transparent, colourless, frothy mucus. The small intestine, which was 141 cm. long, contained 24 c.c. of a brownish fluid, mixed with fragments of several large tape-worms; reaction also alkaline. In the colon and rectum there were only 2 c.c. of a thin brown fluid. The remainder had escaped from the anus while the animal was dying, but was carefully collected by holding a porcelain basin beneath; for during my first experiments I had not employed the method of ligaturing the anus. The evacuated material consisted of a mixture of firm faeces with a large amount of watery liquid, and measured 87 c.c. The colon and rectum were 19 cm. in length. No part of the mucous membrane of the intestines exhibited any congestion which could be attributed to the action of the purgative: a few small reddish spots, but apparently of long standing, and probably caused by the tape-worms, were observable in the upper part of the small intestine; otherwise, the mucous membrane was perfectly pale throughout both stomach and intestines.

The residue obtained after evaporation of the fluid weighed 6·914 grammes, and the ash, less the carbonate of soda, 2·793 grammes. From this ash, which included that of the infusions, was recovered sulphuric acid equivalent to 2·488 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , which corresponds to 1·097 grammes of the water-free salt. Deducting the latter weight from the ash, we obtain 1·696 grammes, or the amount of the ash of the contents of the canal apart from that added by the sulphate. If from 6·914 grammes, the weight of the evaporated residue, be taken 2·488 grammes, or the amount of the crystalline

<sup>1</sup> P. 261.

sulphate of soda recovered, there remain 4·426 grammes, or the real weight of the water-free solids of the fluid. This, multiplied by three, gives approximately the quantity of faecal matter in the colon previous to the administration of the purge, which quantity—reckoning each gramme as equivalent to a cubic centimetre—deducted from the total quantity of the contents found in the alimentary canal, leaves 101·22 c.c. as the amount of fluid due to the presence of the purgative salt. The specific gravity of the faeces, which was in several cases estimated, is so little above that of water, as to justify ignoring the difference in deducting the weight of the faeces from the bulk of the fluid.

This experiment brings out three, perhaps four, noteworthy points. The amount administered of the 5 per cent. solution of the salt has neither increased nor diminished during the hour it was in the alimentary canal. Yet half of the salt has disappeared, proving that absorption must have been active, but equalled by secretion. Further, the mucous membrane is uncongested, and the secretion is therefore not the result of inflammatory irritation. Lastly, deducting the weight of the water-free salt from that of the ash of the intestinal contents, we perceive that there is a remarkably large percentage of ash in the faeces and purgative secretion,—36 per cent. of the solids,—which corroborates the statement I previously made as the result of the first series of experiments, that the saline purgative, apart from its own presence, greatly increases the quantity of salts in the faeces.

*Experiment LX.*—Black cat, male, lean, weighing 2·33 kilogrammes. As the cat was smaller than the last, I administered a less dose. 40 c.c. of a 10 per cent. solution of sulphate of soda, or 4 grammes of the salt, were injected into the stomach. Killed *one hour* afterwards.

*AUTOPSY.*—On opening the abdomen immediately after death, there was no visible outward congestion of the intestines, and no excitement of peritaxis, more than is ordinarily observed after death. The stomach contained 1 c.c. of mucus, and the small intestine 11 c.c. of a brownish viscid fluid, consisting largely of tape-worms mixed with a few undigested shreds of food; and the large intestine contained, including what was evacuated during the death-struggles, 76 c.c. of a mixture of hard lumps of faeces with thin brown fluid of alkaline reaction. The mucous membrane of the duodenum, commencing at the pylorus and extending downwards for about 17 or 18 cm., was marked with a considerable number of little arborescent injections of minute blood-vessels. The small intestine was 116 cm. long, and the large intestine 18 cm.

The evaporated residue of the contents of the intestines, evacuated and unevacuated, weighed 6.157 grammes, and the quantity of sulphuric acid, recovered from this and the infusions of the canal, amounted to 2.454 grammes, calculated as  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ . Deducting the solid contents of the colon, the 40 c.c. of injected salt solution had therefore increased to 76 c.c.

There is much stronger evidence in this than in the preceding experiment that the salt excites secretion. This is dependent on the greater concentration of the solution administered. The 10 per cent. solution has almost doubled itself within the space of an hour, while more than one-third of the salt has been absorbed. The reddish arborescent spots in the duodenum are not to be regarded as due to the action of the salt. They are either caused by the tape-worms present, or by a chronic duodenal congestion which is not uncommon in cats. For, in several animals killed without the previous administration of a saline purgative, or indeed any drug, I have often observed the same appearance, especially when tape-worms were present as they so constantly are. Had the congestion in the last experiment been produced by the salt, it would in all probability have been diffuse and not maculate and arborescent, and would have extended down the jejunum.

*Experiment LXI.*—Cat, male, weighing 2.95 kilogrammes. Administered 25 c.c. of a 20 per cent. solution of sulphate of soda, or 5 grammes of the salt. Killed at the end of one hour.

*AUTOPSY.*—The stomach contained 0.3 c.c. of colourless, viscid, frothy mucus, with an alkaline reaction. The small intestine contained 22 c.c. of a viscid fluid, well mixed with a large quantity of yellowish-white flocculi and fragments of tape-worms, and a few particles of brownish matter. The fluid, where free from flocculi, was transparent and colourless, and apparently contained no bile. The absence of bile was confirmed by the fluid exhibiting no play of colours with strong nitric acid. The reaction of the fluid was alkaline, and 10 c.c. of it required 0.125 c.c. of the pharmacopoeial standard solution of oxalic acid for its neutralisation. The acid solution was diluted as explained in the second series of experiments.

The diastatic power of the fluid was ascertained. 2 c.c. of the transparent colourless portion of the fluid from the small intestine were mixed with a 2 per cent. solution of starch, and allowed to digest for forty-eight hours. Neither of the original fluids contained sugar. At the end of this time the mixture contained 0.265 gramme of maltose, as estimated by Pavy's modified Fehling. 5 c.c. of the intestinal fluid would, therefore, have produced 0.662 gramme of maltose.



5 c.c. of blood were drawn from the vena cava immediately after death, and were also mixed and digested with a 2 per cent. solution of starch. In forty-eight hours 0.411 gramme of maltose was formed. Before estimating the sugar, the albumen was removed by acidification, boiling, and filtration. The intestinal fluid was, therefore, hardly more active than blood in the digestion of starch, and could not have contained more than the merest trace of the pancreatic juice.

As a large discharge had taken place from the rectum immediately previous to death, the colon and rectum were found to be almost empty. Their contents, along with the evacuated fluid, measured 71 c.c., and consisted of the usual mixture of brown solids and fluids; the reaction was neutral. The residue obtained by evaporation weighed 5.767 grammes. The quantity of sulphuric acid, reckoned as  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , recovered from the whole contents of the alimentary canal and from the infusions, accurately allowing for what was used for digestive purposes, amounted to 2.766 grammes. The 25 c.c. of salt solution injected had increased to 84 c.c.

The mucous membrane of the stomach was very slightly congested, but that of the intestines was perfectly pale in its whole extent.

In this experiment the 20 per cent. solution administered has much more than trebled itself. And there cannot now be the shadow of a doubt, when we consider the results of these three experiments, that a solution of sulphate of soda stronger than 5 per cent. excites secretion within the alimentary canal. There is also every reason to assume, from the nearly equal absorption of the salt in all of the experiments, that even a 5 per cent. solution excites secretion almost as copiously as a 20 per cent. solution, although in the former case, on account of the greater dilution of the salt, absorption from the canal is less hindered, and consequently the bulk of the saline solution is not increased as in the latter case. Thus, by the most unobjectionable of methods, I venture to think I have indisputably established that a saline purgative stimulates secretion within the alimentary canal; and it would appear from the last experiment that the secretion is derived from the canal itself and not from the pancreas or bile. The rapidity of the purgative action and of the absorption of the salt is also remarkable.

The next experiments were mainly intended to elicit the course of action of the salt, the cats being killed at various periods after the administration of the salt.

*Experiment LXII.*—Cat, female, weighing 2.60 kilogrammes. Administered 25 c.c. of a 20 per cent. solution of sulphate of soda, or 5 grammes of the salt. Killed *fifteen minutes* afterwards.

**AUTOPSY.**—The abdomen was opened, and ligatures placed round the pylorus, jejunum, and cœcum immediately after the animal was stunned, and before any convulsive spasms had occurred. The stomach contained 29 c.c. of a colourless, slightly opalescent, limpid fluid, *alkaline* in reaction. Its opalescence increased on heating, and the application of other proteid reactions proved the presence of a distinct trace of albumen. The duodenum contained 1 c.c. of a viscid yellowish fluid, and the jejunum 7 c.c. of a colourless, almost transparent liquid. In the ileum were 2 c.c. of a viscid, colourless fluid. The colon contained no fluid, only the usual firm faecal matter. The mucous membrane of the stomach was slightly congested; that of the duodenum and of the upper part of the jejunum, extending for about 42 cm. from the pylorus, was diffusely and markedly congested. Out of many experiments, this is the only occasion on which I observed a diffuse and acute congestion of the intestinal mucous membrane after the action of the salt, even when an equally strong solution was administered, and the animal killed within an equally short period.

The gastric fluid, including an infusion of the stomach-wall, yielded 3.546 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , as calculated from the sulphuric acid recovered. As from other experiments, which will be afterwards given, I learned that the sulphate is absorbed with extreme slowness by the stomach, I concluded that a part of the salt solution had passed into the intestine, even were the presence of fluid there no indication of an escape from the stomach. And it is probable that this escape had taken place immediately before the animal was killed. For, assuming on good grounds that the stomach had not absorbed more than a quarter of a gramme of the salt, and that the remainder of the salt was distributed over 39 c.c. of fluid, or the total quantity found in the stomach and intestines, we would expect to find in 29 c.c. of the fluid, or the quantity in the stomach, 3.54 grammes of the salt, or exactly the amount recovered. And, as from this calculation the intestinal fluid must have contained the same proportion of salt, the salt solution could not have been more than a very few minutes within the intestine, or it would have lost much of its salt by absorption and increased rapidly in bulk, as I have ascertained from previous experiments.

The important conclusion from this experiment is that the salt, when administered in the form of a 20 per cent. solution, is capable of withdrawing from the stomach a tolerably large amount of secretion. The increase to 39 c.c. of the 25 c.c. administered must be mainly attributed to the secretion supplied by the stomach. It is otherwise when the salt solution is not stronger than 10 per cent., as a future experiment will prove.

*Experiment LXIII.*—Cat, male, weighing 4.30 kilogrammes. Injected into the stomach *per os* 25 c.c. of a 20 per cent. solution of

sulphate of soda. Killed *twenty minutes* afterwards. As in the preceding experiment, the abdomen was opened and the canal ligatured at various points immediately after stunning the animal.

**AUTOPSY.**—The stomach contained 1 c.c. of a colourless, transparent, slightly acid fluid, and the small intestine, 9.5 c.c. of a colourless, or faintly yellowish, opaque fluid, with an alkaline reaction. In the large intestine were 79 c.c. of a brownish watery fluid, mixed with hard faecal lumps. The mucous membrane of the stomach was slightly congested, at least was of a dusky hue, towards the pylorus. In the upper part of the small intestine was a number of irregular congested spots or patches, which were evidently chronic in nature. Nearly a dozen of tape-worms were removed from the intestine. The rest of the mucous membrane of the intestines was perfectly pale. The small intestine was 162 cm. long.

The contents of the colon, after evaporation, weighed 6.661 grammes, and, along with the other fluids and with the infusions, yielded 4.526 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , reckoned from the sulphuric acid recovered. The amount of fluid due to the presence of the salt was 74.4 c.c.

The great rapidity with which the salt solution has increased from 25 c.c. to 74 c.c. is very remarkable, this having been accomplished within twenty minutes. On the other hand, comparatively little of the salt has been absorbed. Half a gramme of salt absorbed, and 50 c.c. of fluid secreted, show such a proportion of salt to secretion which quite precludes the supposition of many investigators<sup>1</sup> that the secretion is due to the osmotic power of the salt. The endosmotic equivalent<sup>2</sup> of crystalline sulphate of soda is 11, and half a gramme of the salt should have caused a secretion of only 5 or 6 c.c. The same form of argument against the salt acting osmotically applies with hardly less cogency to Experiment LXII. and even to Experiment LXI.

**Experiment LXIV.**—Cat, female, weighing 2.55 kilogrammes. Administered 25 c.c. of a 20 per cent. solution of sulphate of soda, equivalent to 5 grammes of the salt. Killed *half an hour* afterwards, and the alimentary canal ligatured at pylorus, middle of jejunum, and cæcum, immediately after stunning.

**AUTOPSY.**—The stomach contained 17 c.c. of a clear acid fluid, along with a piece of well-nigh completely digested meat,<sup>3</sup> about twice or

<sup>1</sup> P. 244.

<sup>2</sup> Aubert, *op. cit.*

<sup>3</sup> As I afterwards ascertained, the cat had obtained this by accident early in the morning.

three times the size of a filbert. The duodenum and upper half of the jejunum contained 8 c.c. of a colourless (untinted by bile), opalescent, limpid fluid, of a slightly acid reaction. In the lower part of the jejunum and in the ileum were 14 c.c. of a colourless fluid, mixed with a few brownish particles of ingesta; alkaline in reaction. In the colon and rectum, exclusive of solid faeces, there were 10 c.c. of fluid.

All the fluids, mixed with the infusions of the canal and of the food in the stomach, yielded 3.412 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , estimated from the sulphuric acid present. The 25 c.c. injected had, therefore, increased to 49 c.c.

The mucous membrane was everywhere pale, excepting one or two irregularly disposed and slightly congested spots in the duodenum close by the pylorus.

The increase of the salt solution is not so great as in previous experiments, and has doubtless been modified by the presence of a small quantity of food in the stomach. It is worthy of remark that, while the fluid in the stomach and upper half of the intestine is still acid, it has become alkaline in the lower half. The acidity of the latter has been neutralised by the alkaline intestinal secretion, and not by the bile or pancreatic juice. For if it had been effected by the latter, the change in reaction would have been observable so soon as the fluid had passed beyond the point of entrance of these secretions in the duodenum; and, therefore, the fluid in the upper half of the intestine ought also to have been alkaline. This is another proof of the opinion already advanced, that the bile and pancreatic juice contribute practically nothing to the secretion excited by the salt.

*Experiment LXV.*—Cat, male, weighing 2.95 kilogrammes. Injected into the stomach *per os* 50 c.c. of a 10 per cent. solution of sulphate of soda, or 5 grammes of the salt. Killed *two hours, ten minutes* afterwards.

**AUTOPSY.**—The stomach contained 2 c.c. of a frothy yellowish fluid, alkaline in reaction. In the small intestine were 20 c.c. of thick mucus, largely mixed with tape-worms, and also of a yellowish colour. The contents of the colon, which was fully distended, measured 83 c.c., and consisted in part of hard faecal masses; reaction neutral. Evaporated to dryness, the residue weighed 8.942 grammes, and, with the residues of the other fluids and of the infusions, yielded sulphuric acid equivalent to 3.724 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ . The 50 c.c. originally injected had, therefore, after deducting faeces, increased to 89 c.c.

The mucous membrane of the stomach was covered with viscid yellow mucus, but was quite free from congestion. The small and

large intestines showed also no trace of congestion unless in the upmost 20 cm., where a slight degree of chronic congestion was visible.

Comparing this experiment with Experiment LX., and allowing for the difference in the dose of the salt, we observe that the salt solution has undergone no material change in bulk since the end of one hour, and, it may be, even a much shorter period, after its administration. The maximum of the bulk of the saline fluid within the canal is, therefore, early attained.

The quantity of the salt recovered in this experiment calls much more for our attention. We have formerly seen that the amount of the salt within the canal gradually decreases from the time of its administration until one hour afterwards. In this experiment we find it at the end of two hours on the increase. Instead of the  $2\frac{1}{2}$  grammes, recovered in the three experiments, where the animal was killed at the end of one hour, we have roughly  $3\frac{3}{4}$  grammes. Such a curious and important phenomenon required confirmation, and accordingly the next experiment is simply a repetition of the last.

*Experiment LXVI.*—Cat, male, weighing 3.05 kilogrammes. Administered 50 c.c. of a 10 per cent. solution of sulphate of soda, equivalent to 5 grammes of the salt. Killed after two hours.

*AUTOPSY.*—The intestines, exposed immediately after death, were pale, and exhibited extremely little movement. The stomach contained a drop of yellowish, alkaline mucus; the small intestine 32 c.c., chiefly in the ileum, of a perfectly colourless, limpid, alkaline fluid, with a small deposit of mucus. The colon was distended with 70 c.c. of a brown-coloured mixture of fluid and of solid faeces, also alkaline in reaction. The mucous membrane of every part of the canal was uncongested, unless in the duodenum, 20 cm. of which were marked with a few irregularly disposed patches of chronic congestion. There was a moderate number of tape-worms in the small intestine. The gall-bladder was well distended with bile.

The contents of the intestines yielded, after evaporation to dryness, a residue weighing 10.173 grammes, which, with the infusions, was found to contain 4.078 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , calculated from the sulphuric acid recovered. The fluid originally administered had increased to 83.71 c.c. As the ash of the residue, deducting the sulphates, weighed 1.982 grammes, there must have been a large proportion of salts in the secretion excited by the purgative. The small intestine was 170 cm. in length.

This experiment fully attests the remarkable result of the

previous experiment, exhibiting even a larger amount of salt within the canal after the lapse of two hours.

In order to ascertain, at still later periods, what variations in quantity the salt suffered, was the object of the succeeding experiments.

*Experiment LXVII.*—Cat, male, weighing 2.72 kilogrammes. Administered 50 c.c. of a 10 per cent. solution of sulphate of soda, or 5 grammes of the salt. Killed after *three hours*.

**AUTOPSY.**—On exposing the intestines immediately after death, they appeared abnormally pale. After a few minutes' exposure, however, the blood-vessels ramifying on the peritoneal surface of the gut dilated, and imparted quite a congested aspect to the intestines. I mention this, as it was observed in almost every experiment; and it might lead some other investigator, who neglected to perceive the early uncongested stage, to the belief that the after redness was due to the action of the salt. Vulpian,<sup>1</sup> without doubt, failed to allow for this error in his experiments.

The stomach was practically empty, containing barely a drop of mucus. The contents of the colon and the lower part of the ileum measured 86 c.c. The upper part of the intestine was empty. The mucous membrane of the upper third of the small intestine was slightly congested, especially towards the pylorus, and in appearance and situation the congestion corresponded exactly to what I have so often seen in cats where no salt had been administered.

The fluids and infusions of the whole alimentary canal yielded 2.984 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , reckoned from the sulphuric acid recovered.

From the second to the third hour after its administration the salt apparently begins once more to decrease in amount.

*Experiment LXVIII.*—Cat, male, weighing 2.72 kilogrammes. Administered 50 c.c. of a 10 per cent. solution of sodic sulphate, or 5 grammes of the salt. Killed *four hours* afterwards. Ligatures were placed round the alimentary canal at various points immediately after the animal had been stunned.

**AUTOPSY.**—The stomach contained 4 drops of bilious fluid; the small intestine, 3 c.c. of a yellowish-brown fluid, with an alkaline reaction. In the large intestine were 71 c.c. of a thin, brownish, gruel-like fluid, with the faecal lumps much more disintegrated than in the previous experiments; reaction, neutral or faintly alkaline. The mucous membrane of the small intestine was very slightly and irregularly congested in its upper third. In the rest of its extent both in small and large intestines, it was perfectly pale.

The contents of the intestines, evaporated to dryness, weighed

<sup>1</sup> Vulpian, *op. cit.*

5.242 grammes, and, along with the infusions, yielded 3.072 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , as estimated from the sulphuric acid present. The 50 c.c. of salt solution administered had, therefore, increased to 70 c.c.

This concludes the first portion of the experiments made by this simple method. They were performed with the greatest care, and every precaution was taken to ensure that, in the amount of the recovered sulphate given, all of it that could be procured from the alimentary canal and its contents was represented. They and the remaining experiments of the series were, although they may hardly seem so, the most tedious of all the experiments of this investigation, mainly owing to the time required for the extraction of the salt from the alimentary canal by repeated infusion, and for the perfect drying of the intestinal contents; and, as previously mentioned, the vomiting of the administered salt by the cat, or its being purged before the requisite time had expired, was a frequent source of delay. The results of the experiments are, however, of great interest. The indubitable proof they offer of the salt withdrawing fluid from the blood through exciting secretion in the alimentary canal I have already referred to. And we may hold it as settled that every solution of sulphate of soda stronger than 5 per cent., administered to a cat which has fasted for twenty hours, increases in quantity until its bulk roughly corresponds to the amount of fluid necessary to form a 5 per cent. solution of the dose of the salt originally administered. This maximum is rapidly reached, generally within less than an hour, and begins afterwards to gradually sink, from absorption being more active than secretion.

The secreted fluid was on almost every occasion examined for albumen, and never more than a mere trace could be detected. This is strongly opposed to the secretion being of the nature of an exudation or a transudation. The opposition is strengthened by the uncongested condition of the mucous membrane. The small patchy congestion of the duodenum frequently described is no proof, as I have already discussed, of the irritative action of the salt, as it is often met with in cats to which nothing has been administered, and is evidently either caused by tape-worms, or by repeated physiological arterial engorgements of this part of the canal, or even by a chronic

catarrhal condition. It is obviously chronic. When the congestion was acute and diffuse, as in one experiment (LXII.), I admit the salt may have produced it, but only when in the form of a concentrated solution, as it was in that experiment. And this effect is even exceptional after the application of a 20 per cent. solution, and I have never once observed it after a 10 per cent. solution. The secretion, therefore, cannot be regarded as an inflammatory exudation, as Vulpian has asserted.

The rapid passage of the saline fluid through the alimentary canal is also worthy of observation. Where the animal is fasting, almost the whole fluid reaches the colon within an hour, and at no period is much of it met with in the small intestine. The bulk of it is either in the stomach or the colon. If it has left the former, it passes without much delay through the small intestine to the latter. And if purgation does not at once occur, it is either owing to the inhibitory power exercised by the animal, or to the hardened fæces blocking the rectum and requiring disintegration. The passage of the salt will, doubtless, be much hindered when it is administered with food. The rapid action of a saline purgative, when taken in the early morning before breakfast, is easily understood from these experiments.

Much the most interesting result, however, is the remarkable variations in the quantity of the salt in its passage through the alimentary canal—the rapid absorption of nearly half the salt during the first hour after its administration, and the excretion of a considerable portion of the absorbed salt during the next hour, followed by a steady slow absorption of the salt until defæcation occurs. The annexed table brings this clearly out. In these changes we appear to have confirmation of Headland's statement,<sup>1</sup> now almost forgotten, and perhaps never credited, that the salt is absorbed by the stomach and the upper part of the intestine, and having reached the blood is carried by the circulation through the vessels of the glands of the lower part of the small intestine and colon, and in the act of being excreted by the glands excites them to increased secretion. If Headland's view be correct, we would expect the salt to purge equally, if not more powerfully, when injected directly into the

<sup>1</sup> *Supra*, p. 246.



SHOWING QUANTITY OF SALT AND FLUID WITHIN ALIMENTARY CANAL AFTER ADMINISTRATION OF SULPHATE OF SODA.

No. of Exper.	Weight of Cat.	Dose of Sod. Sulphate.	Strength of Solution of Salt.	Time after Administration.	Quantity of Contents (including Solids) of				Weight of Residue, Evaporated.	Weight of Sod. Sulph. (Na <sub>2</sub> SO <sub>4</sub> ·10H <sub>2</sub> O) recovered.	Weight of Residue, less Sod. Sulph.	Weight of Faecal Matter.	Fluids, less Faecal Matter.	Increase over Fluid administered.
					Stomach.	Small Intestine.	Colon.	Total.						
	kilogr.	grms.	p. c.		c.c.	c.c.	c.c.	c.c.	grms.	grms.	grms.	grms.	c.c.	c.c.
LXII.	2.60	5	20	½ hr.	29	10	...	39	...	3.546 <sup>2</sup>	...	...	39	14
LXIII.	4.30	5	20	20 m.	1	9.5	70	80.5	6.661	4.526	2.035	6.105	74	49
LXIV.	2.55	5	20	½ hr.	17	22	10 <sup>3</sup>	49	...	3.412	...	...	49	24
LIX.	3.28	5	5	1 hr.	1.5	24	89	114.5	6.914	2.488	4.426	13.278	101	1
LX.	2.33	4	10	1 hr.	1	11	76	88	6.157	2.454	3.703	11.109	77	37
LXI.	2.95	5	20	1 hr.	0.3	22	71	93.3	5.767	2.766	3.001	9.003	84	59
LXV.	2.95	5	10	2 hrs.	2	20	83	105	8.942	3.724	5.118	15.354	90	40
LXVI.	3.05	5	10	2 hrs.	0.1	32	70	102	10.173	4.078	6.095	18.285	84	34
LXVII.	2.72	5	10	3 hrs.	0	...	...	86	11.145	2.964	8.161	24.483	62	12
LXVIII.	2.72	5	10	4 hrs.	0.3	3	71	74	5.242	3.572	1.670	4.010	70	20

<sup>1</sup> The quantity of sulphate normally present in the feces is not deducted.      <sup>2</sup> Contents of stomach only.      <sup>3</sup> Without solid feces.

circulation. But the best observers state that purgation does not follow the intravenous injection of the salt, and in the next series of experiments I shall have reason to come to the same conclusion. We are, therefore, precluded from believing that it is the absorbed salt which, in the process of its after excretion, is the cause of the purgative secretion. The only alternative is, that the portion of the salt which causes purgation is that which remains within the alimentary canal, where, by its local stimulus of the glands, it is capable of effecting what it fails to do when passing with the blood through the glands. The absorption and excretion of the salt is a mere concurrent phenomenon which does not concern the essential action of the purgative. Nevertheless, it may be of great importance physiologically, and accordingly I have considered it worth while to determine as far as possible where and how it occurs. With this object I made the following two experiments, in which I ascertained that the rapid absorption of the salt in the early stage of its action does not take place in the stomach.

*Experiment LXIX.*—Cat, female, weighing 3.05 kilogrammes. Opened the abdomen by a short linear incision to the right of and close to the xiphoid cartilage, gently exposed the duodenum and pylorus, ligatured the latter, and afterwards the former below the entrance of the pancreatic and bile ducts, so that in the event of the secretions from the ducts being poured out they would collect between the pylorus and the ligature in the duodenum. The œsophagus in the neck was next exposed, carefully avoiding injury to the vagi and sympathetics, and two ligatures were placed round it, and an incision made into the tube between them. The upper ligature was applied to prevent the escape of saliva. The lower served to prevent the regurgitation of the saline solution after it had been injected into the stomach by means of a tube passing through the œsophageal incision. The stomach was previously ascertained to be empty by gently pressing it after the introduction of the empty tube. The cat, of course, had received no food or water for twenty hours before. 50 c.c. of a 10 per cent. solution of sulphate of soda (5 grammes of the salt) were injected. Killed *one hour* afterwards.

*Autopsy.*—The stomach contained 55 c.c. of a colourless, slightly limpid fluid, faintly opalescent and of *alkaline* reaction. 10 c.c. required 0.14 c.c. of standard solution of oxalic acid for neutralisation. It contained the merest trace of albumen, but gave a dense white precipitate with nitrate of silver and nitric acid, showing that a quantity of chlorides was present. The whole of the fluid, along with the infusion of the stomach, yielded 4.510 grammes of

$\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , reckoned, as usual, from the sulphuric acid recovered.

The duodenal loop was practically empty, containing only one or, at most, two drops of a very viscid bile. The whole of the remainder of the small intestine was perfectly empty, and was pale throughout. The colon contained a small quantity of firm faeces. The gall-bladder was well filled. The urinary bladder contained 8 c.c. of urine, and yielded sulphuric acid equivalent to 0.690 gramme of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ .

*Experiment LXX.*—Cat, female, weighing 2.27 kilogrammes. Operation conducted, and injection made, exactly as in the previous experiment, 50 c.c. of a 10 per cent. solution of sulphate of soda being injected. Killed after the lapse of *three hours and a half*.

*AUTOPSY.*—In the stomach were 69 c.c. of a perfectly colourless, limpid, faintly opalescent fluid, which filtered slowly; reaction, *alkaline*, 10 c.c. requiring 0.11 c.c. of standard solution of oxalic acid for neutralisation. The fluid contained a trace of glucose, and the faintest trace of albumen, when tested by the various reagents; no peptone reaction. It contained a large quantity of chlorides. There was little or no coagulation of mucin with excess of acetic acid. Acidified with hydrochloric acid, it digested fibrin with great facility.

The duodenal loop, as well as the remainder of the small intestine, was perfectly empty. In the upper third of the intestine was a number of irregular patches of congestion. The large intestine contained a small quantity of perfectly firm faecal matter. Gall-bladder, distended with bile.

Half of the fluid from the stomach, with half the infusion of the stomach-wall, contained of sulphuric acid, estimated as  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , 2.042 grammes. The whole contents of the stomach, therefore, contained 4.084 grammes.

From these two experiments we learn that the absorption of the sulphate of soda takes place very slowly from the stomach of the cat, and that a 10 per cent. solution of the salt excites secretion in that organ with great difficulty. The rate of the absorption of the salt and of the increase of the saline fluid is very much less than was observed when the salt was administered *per os*. We cannot, therefore, but conclude that the stomach plays a subsidiary part in purgation, and contributes very little secretion when the solution administered is not stronger than 10 per cent., as is usually the case, although Experiment LXII. renders it probable that a 20 per cent. solution abstracts a fair amount of secretion.

A point of interest, both in these two experiments and in Experiment LXII., is the alkalinity of the gastric secretion. The acid-forming glands are apparently not capable of being stimu-

lated by the salt, although pepsin (Experiment LXX.) was not wanting in the secretion. The presence of pepsin in conjunction with a small amount of mucin would appear to indicate that the secretion is derived as much from the peptic follicles as from the mucous glands. The absence of congestion of the mucous membrane, and of albumen or its digested product, peptones, in the secretion, excludes the probability of the fluid being poured out as an inflammatory exudation from the superficial blood-vessels, without the intervention of the glands, which otherwise the alkalinity of the fluid might indicate, as such an exudation always possesses the alkaline reaction of the blood-serum. The determination of the exact nature of this secretion is of some importance; for it may help us to define the character of the copious secretion resulting from the action of the salt on the intestine. If the gastric secretion had been an ordinary acid gastric juice, we would have expected the intestinal secretion to be a true succus entericus. At least, the acid of the former is wanting, and it raises the possibility of the latter being likewise deficient in some of the constituents of the ordinary succus.

If, as we have just seen, the salt is not rapidly absorbed by the stomach, it appears almost superfluous to add experimental proof of its being much more quickly absorbed by the small intestine. For the rapid absorption in the early stage of the action of the salt must occur somewhere in the alimentary canal, and, if not in the stomach, then in the intestines, and, most probably, in the smaller of these. The next experiment will prove that such is the case.

*Experiment LXXI.*—Cat, male, weighing 2·75 kilogrammes. A short incision was made into the right hypochondrium, and the intestine exposed. Without withdrawing more than an inch or two of the gut, the small intestine was ligatured at the pylorus and close to the cœcum. As both ends of the intestine lay almost immediately beneath the seat of the incision, there was no unnecessary disturbance of the abdominal viscera. As the cat had not received food for nearly twenty-six hours previously, it was certain that the intestine was empty. Through a small incision in the upper end of the duodenum, afterwards ligatured off, 50 c.c. of a 10 per cent. solution of sulphate of soda (5 grammes of the salt) were injected into the small intestine. Killed three hours, thirty-five minutes afterwards.

*AUTOPSY.*—The small intestine was moderately distended with

106 c.c. of a slightly brown, otherwise transparent, fluid; it was somewhat viscid, and contained a few shreddy remnants of food, and whitish flakes. Its reaction was alkaline. There was no yellow tint in the colour of the fluid, and no perceptible bile reaction with the usual reagents.

The stomach was perfectly empty, and the colon contained a quantity of the usual firm faeces, but *not a drop of fluid*.

The mucous membrane of the intestines was pale and uncongested in its whole extent, with the exception of a few centimetres near the pylorus, which showed evidence of a limited chronic congestion.

The fluid from the small intestine yielded, along with the infusion, 2.753 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , as calculated from the sulphuric acid recovered. Half of the fluid and infusion was used for this purpose, the other half of the fluid being reserved for ascertaining its digestive power. 5 c.c. mixed with 10 c.c. of a 1 per cent. solution of starch and placed in the digesting-oven, reduced Fehling after 35 minutes' digestion. The same quantity of the fluid added to 10 c.c. of a 1 per cent. solution of cane-sugar showed the first trace of invert-sugar after one hour, forty-five minutes.

From the comparatively slow digestion of the starch, it is evident that the fluid contained very little pancreatic juice. As the salt solution was injected, and the last ligature applied above the entrance of the bile and pancreatic ducts, the absence of bile, as ascertained by the want of colour in the fluid and the application of reagents, and the absence of pancreatic juice, as discovered from the weak diastatic power of the fluid, confirm the conclusion arrived at from previous experiments, that these secretions do not to any extent form a part of the purgative fluid.

The main result of this experiment is as was anticipated. The absorption of the salt, and the secretion of fluid, are much more rapid in the small intestine than in the stomach; and we may assume that it is in the former these processes chiefly take place when the salt is administered *per os*.

These last three experiments will help us to explain the results obtained from the experiments which preceded them. I have already given one sufficient reason<sup>1</sup> for discrediting Headland's theory of the action of the purgative salt; and to this I have now to add the fact, proved by these experiments, that the absorbed salt never appears accompanied by secretion in a portion of the alimentary canal lower than that into which

<sup>1</sup> P. 582.

it has been injected. A small quantity of salt is absorbed by the stomach, but none of it and no fluid are found, even after more than three hours, in the intestines. A much larger quantity is absorbed by the small intestine, yet none of it reappears in the large intestine, and not a drop of fluid is secreted by that viscus. And I think we may safely take it for granted that, had the salt injected been confined to the upper half of the small intestine, neither salt nor fluid would have been found in the lower half. So that, neither when the salt is directly injected into the blood—the proof of this I have yet to offer—nor when absorbed from the alimentary canal, is it capable of exciting secretion in any alimentary gland through whose vessels it circulates. Local, or, if we might say so, external, contact of the salt and the gland is essential for the stimulation of the latter. Headland adduces in support of his theory Carpenter's<sup>1</sup> single experiment, in which the latter saw purgation follow the injection of sulphate of magnesia into the stomach of a dog with its pylorus ligatured. This is quite opposed to my two experiments, and, as purgation only followed after a considerable time in Carpenter's experiment, it was doubtless due to the mere irritation of the intestines produced by the operation. Carpenter does not appear to have examined the dejection for the salt.

Considering it as fully proved that it is the salt remaining within the alimentary canal which stimulates the glands and purges, we can understand how a quantity of a saline purgative short of a full dose is so little apt to produce a laxative effect commensurate with the quantity of the salt administered, as is the case with most of the vegetable cathartics. A saline purgative, as a rule, either causes a free watery stool or entirely fails to act. If the dose administered be such as not to exceed what can be absorbed from the canal during the stage of rapid absorption, then too little remains within the canal to excite the glands to secretion, and purgation does not occur. On the contrary, constipation is more likely to follow from the diuretic action of the absorbed salt depriving the body of its usual quantity of fluids.

But we have yet to explain how the salt begins to reappear

<sup>1</sup> Carpenter, *op. cit.*

in the canal after its absorption. This cumulative excretion of the salt does not probably occur in the locus of its absorption; for we cannot understand how the same mucous membrane should first quickly absorb the salt, afterwards as quickly excrete it, and, finally, slowly absorb it. The increase of the salt cannot, therefore, well take place in the small intestine, where we believe absorption mainly happens. And this supposition receives support from the last experiment, in which the quantity of the recovered salt was such as to indicate that absorption had not been followed by excretion. We are, accordingly, forced to conclude that the salt gains in quantity after the saline fluid has reached the large intestine, and that this is dependent on a difference in the structure and function of the large intestine as compared with the small intestine. If a solution of a salt be injected into the small intestine, the rapidity of the disappearance of the salt will depend on two factors, absorption and secretion. We have reason to believe, from the B. Series of Experiments, that, however dilute the saline solution may be, and whatever change it may undergo in bulk, both processes of absorption and secretion are very active; and it is reasonable to suppose that their activity will finally lead to an equal proportion or percentage of the salt in the blood and in the fluid within the intestine. This will certainly happen, if the fluid absorbed contain the same proportion of the salt as the intestinal fluid from which it is drawn, and if the fluid secreted contains the same proportion of the salt as the blood. The proportion of salt in the absorbed fluid will gradually grow less, whilst its proportion in the secreted fluid will gradually increase, until both are equal, when the blood and intestinal fluid will contain a like percentage of the salt, and the absolute quantity of the salt in each will be respectively as their bulks. If this be so, then, after the administration of a solution of 5 grammes of sulphate of soda to a cat weighing about 3 kilogrammes, a dose which we know from experiment to increase the fluid within the alimentary canal to about 100 c.c., we would expect to find in the blood two-thirds, and in the intestinal fluid one-third, of the salt given, reckoning that one-fifteenth part of the weight of the cat, or 200 grammes, is blood. But in none of the experiments recorded is the minimum of salt within the canal so low

as one-third, not even in Experiment LXXI., where the salt was permitted to remain for some hours in the intestine, and where ample time was allowed for the blood being recouped by the tissue-fluids for what serum it may have yielded to the intestine. Therefore, either the secretion excited by the salt contains a larger proportion of the salt than the blood, or the absorbed fluid contains a less proportion than the fluid within the intestine. The former alternative is improbable; for no secretion, physiological or pathological, in the body contains a higher percentage of salts than the blood from which it is derived. The urine is a notable exception; but according to Ludwig's well-known theory, the excess of the salts is explained by the absorption in the tubuli uriniferi of a portion of the water of the glomerular secretion. The latter alternative is not without probability. The villi of the intestine may exercise a certain power of selection in absorption, and absorb from a saline solution more of the water and less of the salt than the proportion in which they exist in the solution. This supposition is rendered possible by what we know of the absorption of saline solutions by the roots of plants,<sup>1</sup> and it is supported by experiments on the imbibition of saline solutions by animal tissues. Ludwig<sup>2</sup> observed that a piece of dried urinary bladder, placed in a 7·2 per cent. solution of sulphate of soda, became saturated with a fluid containing only 4·4 per cent. of the salt. Indeed, absorption seems, for the most part, to be governed by the laws which regulate imbibition and diffusion, secretion by the laws of filtration.

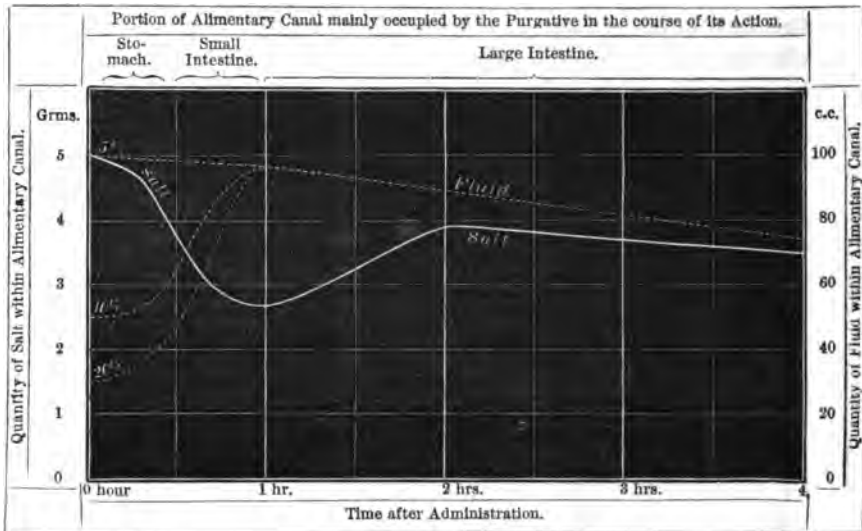
If the small intestine exhibit in its absorption of a solution of sulphate of soda a preference for the water, we have only to believe, in order to understand the rapid accumulation of the salt within the alimentary canal during the second hour of the action of the salt, that the preference for the water and the rejection of the salt are manifested to a still greater degree in the absorptive function of the large intestine. So that, when the saline fluid passes into the colon, a secretion with the same percentage of the salt as the blood will be poured out, just as it was in the small intestine; but in the absorbed fluid the

<sup>1</sup> Saussure, *Recherch. chimiq. sur la végétat.*

<sup>2</sup> Ludwig, *Lehrbuch der Physiologie*, Bd. i. s. 54 et seq.



percentage of the salt will be much less than in the intestinal fluid, and still less than that in the fluid absorbed by the small intestine. The salt will thus quickly accumulate within the colon.



Graphic Representation of the Course of the Variations in the Quantity of Salt and Fluid within the Alimentary Canal after the administration of a 5, 10, or 20 per cent. solution of 5 grms. of Crystalline Sulphate of Soda to a Cat. The salt is calculated from the sulphuric acid recovered.

In this way it is possible to offer an explanation of a phenomenon, or a series of phenomena, otherwise very difficult to comprehend. In accordance with this view I have prepared a diagram which shows the variations of the salt in its passage through the alimentary canal, and which attempts to define in what portion of the canal each of the variations occur.

#### SERIES OF EXPERIMENTS, E.

On the effect of the salt when injected into the blood.

Considering the large and weighty array of investigators who have affirmed that the intravenous injection of a saline purgative is not followed by its usual cathartic action, it seems almost unnecessary that I should have made further experiments to determine the accuracy of this statement. Amongst others,

circulation. But the best observers state that purgation does not follow the intravenous injection of the salt, and in the next series of experiments I shall have reason to come to the same conclusion. We are, therefore, precluded from believing that it is the absorbed salt which, in the process of its after excretion, is the cause of the purgative secretion. The only alternative is, that the portion of the salt which causes purgation is that which remains within the alimentary canal, where, by its local stimulus of the glands, it is capable of effecting what it fails to do when passing with the blood through the glands. The absorption and excretion of the salt is a mere concurrent phenomenon which does not concern the essential action of the purgative. Nevertheless, it may be of great importance physiologically, and accordingly I have considered it worth while to determine as far as possible where and how it occurs. With this object I made the following two experiments, in which I ascertained that the rapid absorption of the salt in the early stage of its action does not take place in the stomach.

*Experiment LXIX.*—Cat, female, weighing 3.05 kilogrammes. Opened the abdomen by a short linear incision to the right of and close to the xiphoid cartilage, gently exposed the duodenum and pylorus, ligatured the latter, and afterwards the former below the entrance of the pancreatic and bile ducts, so that in the event of the secretions from the ducts being poured out they would collect between the pylorus and the ligature in the duodenum. The œsophagus in the neck was next exposed, carefully avoiding injury to the vagi and sympathetics, and two ligatures were placed round it, and an incision made into the tube between them. The upper ligature was applied to prevent the escape of saliva. The lower served to prevent the regurgitation of the saline solution after it had been injected into the stomach by means of a tube passing through the œsophageal incision. The stomach was previously ascertained to be empty by gently pressing it after the introduction of the empty tube. The cat, of course, had received no food or water for twenty hours before. 50 c.c. of a 10 per cent. solution of sulphate of soda (5 grammes of the salt) were injected. Killed *one hour* afterwards.

*AUTOPSY.*—The stomach contained 55 c.c. of a colourless, slightly limpid fluid, faintly opalescent and of *alkaline* reaction. 10 c.c. required 0.14 c.c. of standard solution of oxalic acid for neutralisation. It contained the merest trace of albumen, but gave a dense white precipitate with nitrate of silver and nitric acid, showing that a quantity of chlorides was present. The whole of the fluid, along with the infusion of the stomach, yielded 4.510 grammes of

$\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , reckoned, as usual, from the sulphuric acid recovered.

The duodenal loop was practically empty, containing only one or, at most, two drops of a very viscid bile. The whole of the remainder of the small intestine was perfectly empty, and was pale throughout. The colon contained a small quantity of firm faeces. The gall-bladder was well filled. The urinary bladder contained 8 c.c. of urine, and yielded sulphuric acid equivalent to 0.690 gramme of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ .

*Experiment LXX.*—Cat, female, weighing 2.27 kilogrammes. Operation conducted, and injection made, exactly as in the previous experiment, 50 c.c. of a 10 per cent. solution of sulphate of soda being injected. Killed after the lapse of *three hours and a half*.

*AUTOPSY.*—In the stomach were 69 c.c. of a perfectly colourless, limpid, faintly opalescent fluid, which filtered slowly; reaction, *alkaline*, 10 c.c. requiring 0.11 c.c. of standard solution of oxalic acid for neutralisation. The fluid contained a trace of glucose, and the faintest trace of albumen, when tested by the various reagents; no peptone reaction. It contained a large quantity of chlorides. There was little or no coagulation of mucin with excess of acetic acid. Acidified with hydrochloric acid, it digested fibrin with great facility.

The duodenal loop, as well as the remainder of the small intestine, was perfectly empty. In the upper third of the intestine was a number of irregular patches of congestion. The large intestine contained a small quantity of perfectly firm faecal matter. Gall-bladder, distended with bile.

Half of the fluid from the stomach, with half the infusion of the stomach-wall, contained of sulphuric acid, estimated as  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , 2.042 grammes. The whole contents of the stomach, therefore, contained 4.084 grammes.

From these two experiments we learn that the absorption of the sulphate of soda takes place very slowly from the stomach of the cat, and that a 10 per cent. solution of the salt excites secretion in that organ with great difficulty. The rate of the absorption of the salt and of the increase of the saline fluid is very much less than was observed when the salt was administered *per os*. We cannot, therefore, but conclude that the stomach plays a subsidiary part in purgation, and contributes very little secretion when the solution administered is not stronger than 10 per cent., as is usually the case, although Experiment LXII. renders it probable that a 20 per cent. solution abstracts a fair amount of secretion.

A point of interest, both in these two experiments and in Experiment LXII., is the alkalinity of the gastric secretion. The acid-forming glands are apparently not capable of being stimu-

lated by the salt, although pepsin (Experiment LXX.) was not wanting in the secretion. The presence of pepsin in conjunction with a small amount of mucin would appear to indicate that the secretion is derived as much from the peptic follicles as from the mucous glands. The absence of congestion of the mucous membrane, and of albumen or its digested product, peptones, in the secretion, excludes the probability of the fluid being poured out as an inflammatory exudation from the superficial blood-vessels, without the intervention of the glands, which otherwise the alkalinity of the fluid might indicate, as such an exudation always possesses the alkaline reaction of the blood-serum. The determination of the exact nature of this secretion is of some importance; for it may help us to define the character of the copious secretion resulting from the action of the salt on the intestine. If the gastric secretion had been an ordinary acid gastric juice, we would have expected the intestinal secretion to be a true succus entericus. At least, the acid of the former is wanting, and it raises the possibility of the latter being likewise deficient in some of the constituents of the ordinary succus.

If, as we have just seen, the salt is not rapidly absorbed by the stomach, it appears almost superfluous to add experimental proof of its being much more quickly absorbed by the small intestine. For the rapid absorption in the early stage of the action of the salt must occur somewhere in the alimentary canal, and, if not in the stomach, then in the intestines, and, most probably, in the smaller of these. The next experiment will prove that such is the case.

*Experiment LXXI.*—Cat, male, weighing 2·75 kilogrammes. A short incision was made into the right hypochondrium, and the intestine exposed. Without withdrawing more than an inch or two of the gut, the small intestine was ligatured at the pylorus and close to the cœcum. As both ends of the intestine lay almost immediately beneath the seat of the incision, there was no unnecessary disturbance of the abdominal viscera. As the cat had not received food for nearly twenty-six hours previously, it was certain that the intestine was empty. Through a small incision in the upper end of the duodenum, afterwards ligatured off, 50 c.c. of a 10 per cent. solution of sulphate of soda (5 grammes of the salt) were injected into the small intestine. Killed *three hours, thirty-five minutes* afterwards.

*Autopsy.*—The small intestine was moderately distended with

106 c.c. of a slightly brown, otherwise transparent, fluid; it was somewhat viscid, and contained a few shreddy remnants of food, and whitish flakes. Its reaction was alkaline. There was no yellow tint in the colour of the fluid, and no perceptible bile reaction with the usual reagents.

The stomach was perfectly empty, and the colon contained a quantity of the usual firm fæces, but *not a drop of fluid*.

The mucous membrane of the intestines was pale and uncongested in its whole extent, with the exception of a few centimetres near the pylorus, which showed evidence of a limited chronic congestion.

The fluid from the small intestine yielded, along with the infusion, 2.753 grammes of  $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$ , as calculated from the sulphuric acid recovered. Half of the fluid and infusion was used for this purpose, the other half of the fluid being reserved for ascertaining its digestive power. 5 c.c. mixed with 10 c.c. of a 1 per cent. solution of starch and placed in the digesting-oven, reduced Fehling after 35 minutes' digestion. The same quantity of the fluid added to 10 c.c. of a 1 per cent. solution of cane-sugar showed the first trace of invert-sugar after one hour, forty-five minutes.

From the comparatively slow digestion of the starch, it is evident that the fluid contained very little pancreatic juice. As the salt solution was injected, and the last ligature applied above the entrance of the bile and pancreatic ducts, the absence of bile, as ascertained by the want of colour in the fluid and the application of reagents, and the absence of pancreatic juice, as discovered from the weak diastatic power of the fluid, confirm the conclusion arrived at from previous experiments, that these secretions do not to any extent form a part of the purgative fluid.

The main result of this experiment is as was anticipated. The absorption of the salt, and the secretion of fluid, are much more rapid in the small intestine than in the stomach; and we may assume that it is in the former these processes chiefly take place when the salt is administered *per os*.

These last three experiments will help us to explain the results obtained from the experiments which preceded them. I have already given one sufficient reason<sup>1</sup> for discrediting Headland's theory of the action of the purgative salt; and to this I have now to add the fact, proved by these experiments, that the absorbed salt never appears accompanied by secretion in a portion of the alimentary canal lower than that into which

<sup>1</sup> P. 582.

The urine collected during the first twenty-four hours gave a distinct sugar reaction, but contained no albumen.

In conjunction with the results of previous observers we are perfectly warranted in concluding from these three experiments that the sulphate of soda is quite incapable of inducing its usual purgative effect when injected into the circulation. On the contrary, a degree of constipation appears usually to follow.

A somewhat remarkable phenomenon observed in the course of these and similar experiments is the appearance of sugar (glucose presumably) in the urine. On referring to Hermann's *Physiology*, I find that Bock and Hoffmann are credited with the statement that very dilute saline solutions cause, when injected into the blood, a temporary diabetes mellitus. I have more than once observed the same result follow the injection of the salt into a ligatured loop of intestine. It is not improbable that the salt in the blood may hinder the assimilation of the carbo-hydrate by the tissues, while its production in the liver proceeding as usual may lead to its accumulation in the blood and its discharge in the renal secretion.

Sulphate of magnesia was employed in the next experiments, the main interest of which lies in their proof of the high toxic power possessed by this salt as compared with the corresponding salt of soda. This toxicity is but very vaguely understood and not generally known, scarcely one of the best text-books on pharmacology alluding to it with emphasis.

*Experiment LXXV.*—Dog, weighing 7.25 kilogrammes. Anæsthetised. During the injection of a 20 per cent. solution of sulphate of magnesia into the external saphenous vein of the left hind leg, and when not more than 3 c.c. had been injected, the animal died, and animation could not be restored by means of artificial respiration. Death may have resulted from an over-dose of the anæsthetic, but this is improbable. It was more likely due to the sudden injection of the concentrated solution of the salt, for death immediately followed the injection. The quantity of the salt—0.6 gramme—was extremely small, and was not more than the twentieth part of a purgative dose.

Not being aware, as yet, of the high toxic power of this salt, I attributed the death of the dog to the anæsthetic, and proceeded to repeat the experiment on a cat.

*Experiment LXXVI.*—Cat, female, weighing 2.28 kilogrammes.

<sup>1</sup> Hermann's *Elements of Physiology*, translated by Gamgee, 1875, p. 189.

Lightly anaesthetised. Not more than 1·5 c.c. of a 20 per cent. solution of sulphate of magnesia had been leisurely injected into the saphenous vein, when the cat died as suddenly as the dog had done.

As I now suspected that the sudden and fatal termination of these two experiments was due to the salt, I took another cat, and injected a 10 per cent. solution very slowly and cautiously, so as to obviate as far as possible the directly and locally paralytic effect of the salt on the heart from possible irritation of the endocardium. I need hardly state that I ascertained that the salt used was chemically pure.

*Experiment LXXVI.*—Cat, female, weighing 2·26 kilogrammes. A 10 per cent. solution of sulphate of magnesia was injected into the left external saphenous vein, and injected so slowly that twenty minutes were occupied in introducing 6 c.c. When this point had been reached respiration altogether ceased, while the heart continued to beat, although with greatly diminished rapidity. Sylvester's method of artificial respiration was maintained for a few minutes, until natural respiration recommenced. On account of the animal having appeared perfectly unconscious since a few minutes after the commencement of the injection of the salt, no anaesthetic had from that time been applied. During the forty minutes succeeding the recovery of the animal from suspended respiration 7 c.c. of the saline solution were gradually injected. This was followed once more by cessation of respiration, the heart beating irregularly at the rate of 31 per minute. Previous to the experiment it was 146. Artificial respiration was at once resorted to, and maintained for ten minutes, but ineffectually. The heart meanwhile was beating very slowly, and gradually becoming weaker until it finally stopped, without a single effort having been made by the animal to respire.

*AUTOPSY.*—The abdomen was immediately opened, and the intestines observed to be quite at rest, and perfectly empty, with the exception of the colon, which contained the usual semi-solid faeces. The mucous membrane of the canal was pale, unless for 20 cm. below the pylorus, where there existed small irregular and moderately congested patches, evidently of a chronic character. There were no tape-worms. The urine did not contain much magnesia.

This experiment rendered it perfectly clear that sulphate of magnesia possessed considerable toxic power; and from the care employed in its injection it was highly probable that the amount of the salt used—1·3 grammes—was the lethal dose for a cat weighing 2·26 kilogrammes. This quantity is much larger than that needed in the two previous experiments, but in these it is almost certain that the greater concentration of the saline

solution and its more rapid injection directly affected the heart and respiration.

Having now ascertained the lethal dose of sulphate of magnesia, which, it will be observed, is far short of the dose required *per os* for purgation, I proceeded to inject into the circulation of another cat as much of the salt as I possibly could short of causing death, with the object of learning if purgation might follow.

*Experiment LXXVII.*—Cat, female, weighing 1.81 kilogrammes. Anæsthetised. A 5 per cent. solution of sulphate of magnesia was injected into the external saphenous vein of the left hind leg.

Time.	Quantity of Salt Solution injected.	Pulse per Minute.	Respirations per Minute.
4.10 P.M.	...	128	56
4.15 to 4.16	2 c.c.	...	...
4.20 <sup>1</sup>	...	140	32
4.21 to 4.32	6 c.c.	...	...
4.35 <sup>2</sup>	...	108	26
4.36 to 4.38	2 c.c.	...	...
4.41	...	104	24
4.42 to 4.47	2.5 c.c.	...	...
4.50	...	84	20
4.51 to 4.57	2.5 c.c.	...	...
5	...	76	18
5.1 to 5.7 <sup>3</sup>	5 c.c.	...	...
5.10	...	76	17
5.12	1 c.c.	...	...
5.13	...	{ 40-52 irregular.	very slow and deep.
5.13½	2 c.c.		

5.14.—Respiration ceased, followed in fifteen seconds by the stoppage of the heart. Sylvestrian artificial respiration was at once practised, and one minute afterwards the heart commenced to beat, but very slowly. Four minutes later, natural respiration returned, and artificial respiration was stopped.

5.24.—Again stoppage of the respiration, followed by that of the heart. Artificial respiration was once more resorted to.

<sup>1</sup> The application of the anæsthetic was discontinued, as the animal remained unconscious without it.

<sup>2</sup> From this time the conjunctivæ were quite insensible to stimulus, nor could pinching of any part of the body provoke a reflex movement. The animal appeared to be in a state of profound narcosis.

<sup>3</sup> At 5.4 there was a gentle movement of the whole body, and slight reflex sensibility of conjunctivæ.



5.26.—The heart's action restored, and four minutes afterwards the pulse was 40.

5.37.—Artificial respiration stopped; and, although the heart continued to beat, it was not until three or four minutes afterwards that natural respiration attempted to reassert itself by an occasional upheaval of the upper part of the chest, the diaphragm remaining apparently quite inactive. Respiration was gradually established. Reflex sensibility of every part of the body still completely abolished.

5.43.—Pulse 42, and respirations 8.

5.48.—Pulse 48, and respirations 10.

The wound was now closed, and the cat was removed from the holder. It was still perfectly insensible to stimulation. The pupils contracted very slowly on exposure to light, and when contracted their dilatation in the dark occupied several minutes, an operation accomplished almost momentarily in the normal cat.

5.55.—Pulse 48, and respirations 12. Three minutes later the cat made a few feeble and very limited spontaneous movements.

6.5.—Constant small twitching movements of all the limbs commenced. Conjunctivæ still insensible. Pulse now 64, and respirations 18.

6.25.—The twitchings had ceased, and there were instead occasional movements of the head.

6.45.—Pulse 80, and respirations 22; conjunctivæ not yet sensible.

7.5.—Pulse 102, and respirations 32. Conjunctivæ had now become slightly sensible, and the animal made fair co-ordinate efforts to move.

8.0.—Pulse 104, and respirations 24; able to sit up, but very weak. Conjunctivæ quite sensible.

9.0.—Pulse 104, respirations 24; still very helpless, although a little stronger.

Next day it looked as well and as lively as it did before the experiment, and enjoyed its food. Pulse 192, respirations 48 to 56. It appeared to be very thirsty during the day. No fæces passed as yet.

On the third day there was a small evacuation of hard fæces in the afternoon.

Altogether, 23 c.c. of the salt solution, or 1.15 grammes of the salt, were injected.

This experiment fully warrants the deduction that sulphate of magnesia, no more than sulphate of soda, is capable of purging when injected intravenously. There is, however, this important difference, that whereas a full purgative dose of the latter salt can be introduced with impunity into the circulation, not more than one-fifth part of an ordinary dose of the former can be injected without endangering the life of the animal. And it is possible that, could more of the sulphate of magnesia be introduced into the blood, it might affect the intestines, and produce

catharsis. But this is in the highest degree improbable, as in Experiment LXXVI., where the animal died some time after the injection of the salt, not the least sign of cathartic action was visible in the intestines.

If it be admitted, then, that a saline cathartic does not produce its characteristic action when injected into the blood, we have proved a fact which is not at variance with the results of the previous series of experiments, or with the theories I have attempted to base upon them. We are, therefore, now entitled to dismiss Headland's theory, that it is the salt which is absorbed by the blood that causes purgation in the course of its excretion by the intestinal glands. We cannot, however, so easily rid ourselves of the results of the experiments upon which, in part, his theory was grounded. It will be remembered, that in these experiments Headland administered 180 grains (about  $11\frac{1}{2}$  grammes) of sulphate of magnesia to each of three dogs, and that from the alimentary canal of one of the animals he recovered less than one-third of the salt; so that about  $7\frac{1}{2}$  grammes had evidently been absorbed by the blood. The question naturally arises, if this be so, why were none of the toxic symptoms of the magnesia salt witnessed in the animals to which it had been administered; and why, indeed, were none of these symptoms observable in the various animals to which in the course of this investigation I gave large doses of the salt? The absence of such symptoms might be held as proof that no such absorption had occurred; or it might be suggested that dogs, the only animals used by Headland, are not amenable to the poisonous action of magnesia. But, that such an absorption did occur, is placed almost beyond doubt by my more numerous experiments on cats with the sulphate of soda (Series D.), in which it was observed that more than  $2\frac{1}{2}$  grammes, or the half of the salt administered, had disappeared within the first hour from the alimentary canal. Nor, apart from the improbability of the suggestion, are dogs less susceptible than cats of the toxic action of magnesia; for, excluding my single experiment in which a dog died after the injection of 0.6 gramme of the sulphate, Jolyet and Cahours<sup>1</sup> seriously imperilled the life of a dog by injecting into its circulation 6 grammes of the same salt. Yet  $7\frac{1}{2}$  grammes must have been absorbed in Headland's experi-

<sup>1</sup> *Op. cit.*

ment without the slightest symptom of poisoning. In reconciling the results of the experiments of this series and of those of the preceding series, we have to face a difficulty of considerable magnitude. Sulphate of magnesia produces well-marked symptoms when directly injected, even in very small quantity, into the blood. Yet the same salt, when absorbed from the alimentary canal, and in comparatively large quantity, causes no general symptoms whatever. Either the salt which disappears from the canal does not enter the blood, or it enters the blood in such a form or chemical combination that it is no longer capable of affecting the activity of the tissues and organs with which it comes in contact. Analyses of the blood are, as yet, wanting to test the probability of the latter supposition; these I hope ere long to supply. Should the former alternative prove true, a still greater difficulty presents itself, when we come to consider by what channel, or into what organ, the salt has disappeared. In so far as our present knowledge of anatomy and physiology can help us, we can only think of its being absorbed by the liver from the blood of the portal vein, and restored to the intestines through the medium of the biliary secretion; for, both according to Headland's experiments and to mine, the salt after its first rapid absorption returns to the alimentary canal. But my observations have led me to conclude that the bile takes little or no part in the formation of the fluid of saline catharsis; in D. Series of Experiments a mere tinging of the intestinal contents with bile was of rare occurrence. On the other hand, the recent researches of Héger and Jacque<sup>1</sup> have proved that the liver is capable of absorbing a large proportion of certain alkaloids, when injected into the mesenteric vein, as much as 25 to 50 per cent. of nicotin having been absorbed in this manner. Their elimination in the bile they have not quite so satisfactorily demonstrated. On the whole, I prefer to express the opinion, that whatever be the circuit traversed by the salt after leaving the alimentary canal, it is not through the liver and gall-bladder. The solution of this difficulty requires further research.

Apart from the bearing of these experiments with sulphate of magnesia on the nature of the purgative action of the salt, they

<sup>1</sup> Héger et Jacque, *Gazette hebdom.*, No. 29, 1880.

are of interest as tracing with some precision the effect of the salt on the circulation and respiration when injected into the blood. The pulse is at first slightly accelerated, but soon begins to diminish in rapidity until, following the stoppage of the respiration, it ceases to be felt. The respirations are from the first lessened in frequency, and it is to the paralysis of their motor centre that death is to be attributed. The action of the salt on the respiratory centre is not, however, permanent; for, if artificial respiration be maintained for a few minutes, until the excess of the salt has been eliminated from the blood, or otherwise rendered less active, natural respiration will reassert itself, although with greatly enfeebled activity. The fact of the heart continuing to beat throughout the maintenance of the artificial respiration, and while natural respiration remains as yet paralysed, points to the salt acting more powerfully on the respiration than on the heart. A remarkable feature is the abolition of reflex sensibility, which is complete within twenty minutes after the commencement of the injection, and when not more than 0·4 gramme of the salt has been administered, and which continues for one hour and a half after the paralysed respiration has been re-established, and for more than one hour and three quarters after the injection of the salt has been completed. This affection of the reflex sensibility seems to depend upon paralysis of the sensory nerves, or of the reflex centres, not upon paralysis of the motor nerves or the muscles. For voluntary movements of the body are visible long before reflex sensibility has been restored. Altogether the action of magnesia is remarkable, and precludes its being classed with any recognised metallic pharmacological group.

Without taking into consideration the more puzzling points which I have already endeavoured to elucidate, it may appear somewhat surprising that so large a dose of sulphate of magnesia can be administered by the mouth with absolute safety, whilst a fraction of this dose is sufficient to produce the serious symptoms I have described, and even death, when injected directly into the blood. This pharmacological paradox is not singular; for, not to speak of the well-known instance of curara, the salts of potash show a similar behaviour. The potash salts are, when injected into the blood, nearly four or five times as poisonous as the magnesia

salts;<sup>1</sup> yet an ounce of the sulphate of potash may be swallowed by a man, or 8 to 10 grammes administered to a cat, without causing more than free purgation. The explanation hitherto given has been that, owing to the elimination of the substance by the kidney being as rapid as its absorption from the alimentary viscera, it does not accumulate in the blood in quantity sufficient to produce its general toxic symptoms. Accepting as accurate the results of Headland's and my experiments, this explanation is obviously not applicable to the salts of magnesia; and as little may it be the true explanation of the behaviour of the potash salts.

Amidst much that is uncertain, there is but one sure conclusion that can be inferred from these experiments in conjunction with those of the various preceding series, that it is not the absorbed salt which causes purgation by stimulation of the intestinal glands in the process of its excretion, but it is the salt remaining within the intestines which excites the glands; and the reappearance of the salt in the intestines is not the cause of the secretion, but merely the accompaniment of it.

In the following experiment an artificial imitation was attempted of the supposed relative distribution of the sulphate of soda in the blood and alimentary canal one hour after the administration of a purgative dose *per os*, with the object of ascertaining if, under these circumstances, purgation would follow.

*Experiment LXXVIII.*—Black cat, female, weighing 3·02 kilogrammes. 25 c.c. of a 10 per cent. solution of sulphate of soda, or  $2\frac{1}{2}$  grammes of the salt, were first administered in the ordinary manner, to observe if this quantity of itself was sufficient to purge. No purgation followed. Three days afterwards, 3 grammes of the salt (20 per cent. solution) were injected into the external saphenous vein of the left hind leg. The injection extended over ten minutes, and the animal was anaesthetised. Immediately afterwards, it was released from the holder, and  $2\frac{1}{2}$  grammes of the salt (10 per cent. solution) were injected *per os* into the stomach. Purgation never occurred, although the animal was observed for two days afterwards.

The artificial distribution of the salt in the blood and the alimentary canal was not, therefore, followed by purgation, as I had partly anticipated. On a point like this a single experiment is not conclusive. But, supposing that the result is exactly as

<sup>1</sup> Mickwitz found that the intravenous injection of 0·2 gramme of nitrate of potash killed a cat.—*Nothnagel u. Rossbach's Arzneimittellehre*, 1878, s. 14.

it should be, then I either failed to imitate exactly the condition of the salt, when all of it had been administered by the mouth, or some factors came into operation in this experiment which might have interfered with the effect of the salt. As regards the latter, the cat took almost no food and very little water for two days previous to the experiment, and this may have led to concentration of the blood, a condition which I have proved to interfere materially with the purgative action of the salt. As concerns the former, I failed to imitate perfectly the condition of the salt in one important respect, inasmuch as the portion of the salt administered by the mouth was at the upper end of the alimentary canal, and not at its lower end, as is the case one hour after the administration of the whole dose of the salt by the mouth, and it had to traverse the length of the highly absorptive small intestine, where possibly so much of it was absorbed by the blood that too little remained in the intestines to stimulate sufficiently the enteric glands. The incorrectness of the imitation may have been still more gross. For there is the possibility, previously alluded to, that the salt which disappears from the canal may not be found in the systemic circulation. In such a case a negative result was quite to be expected.

TABULATED SUMMARY OF THE EXPERIMENTS OF SERIES D.

Number of Experiment.	Animal.	Weight of Animal.	Salt employed.	Quantity of Salt injected.	Remarks.
		kilograms.		grms.	
LXXII.	Rabbit	1.58	Sulphate of soda	5	{ 150 c.c. of water injected <i>per os</i> : constipation. Afterwards killed: no signs of purgation.
LXXIII.	Cat	1.81	"	5	
LXXIV.	"	2.03	"	6	Constipation.
LXXV.	Dog	7.25	{ Sulphate of } { magnesia }	0.6	Died suddenly.
LXXVI.	Cat	2.26	"	1.3	{ Died after one hour: no signs of purgation. Constipation.
LXXVII.	"	1.81	"	1.15	
LXXVIII.	"	3.02	Sulphate of soda	3	{ 3 grms. also given <i>per os</i> : no purgation.

(To be continued.)

A VARIETY OF PULMONARY LOBATION AND ITS  
RELATIONS TO THE THORACIC PARIETES, AS  
ILLUSTRATED BY COMPARATIVE ANATOMY AND  
ABNORMALITIES IN THE HUMAN SUBJECT. By  
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IN human anatomy what are known as pulmonary lobes are the parts into which each lung is divided by the fissures which pass inwards from the surface of the viscus. Occasionally in man these lines of fission are superabundant; thus, instead of the normal number of two fissures for the right lung and one for the left, there may be three and two fissures respectively. But it is not only by multiplication of fissures, similar to those which occasionally occur, that the lobes may be increased in number. The abnormal pulmonary lobes dealt with in the present communication are interesting, in that they are not the result of parenchymal fission, but depended for their production on a mechanical cause, of which a constricting, cord-like action of vascular trunks on a soft tissue is the essential factor.

Before entering into a description of these human abnormalities, an inquiry into the conditions of the thoracic viscera in lower mammals will be useful, especially as in these a relationship seems to exist between the peculiarities of the lungs and the shape of the chest, and the investigation of them will prepare us to explain the abnormal conditions of the human thorax.

On comparison of the human thorax with that of any of the domestic mammals—*e.g.*, the horse—it is found to have quite a different shape, whereby it is adapted to the erect posture. In man the thorax has the transverse diameter greater than the sterno-vertebral, and is remarkably short; whilst in the generality of prone mammals it is greatly elongated, and has the sterno-vertebral greater than the transverse diameter. As a consequence of greater length of thorax, the diaphragm and pericardium are separated from one another, and are connected only by reflections of pleura, between the layers of which there may be detected

aponeurotic bands of a ligamentous description.<sup>1</sup> Correlated with separation of the diaphragm from the pericardium there are other peculiarities, thus:—The posterior vena cava has its intra-thoracic stage of great length, and surrounded by the right layer of mediastinal pleura, which in most cases projects in the form of a pouch above and to the left of a vein, something like the lesser sac of the peritoneum, and comes in contact not only with the large serous sac from which it proceeds, but also with the pleura of the opposite side.<sup>2</sup> The membranes thus brought into contact lie back to back, and form the connections between the diaphragm and pericardium already noted. The sac also lies against the posterior part of the pericardium, and touches posteriorly the diaphragm. Into it fits a small elongated lobe, which originates from the inner side of the right lung near its base. This lobe is spoken of in works on comparative anatomy under various designations, viz., the *lobulus impar*, *lobulus azygos*, and *the lobule of the vena cava*.<sup>3</sup> Of all these names the latter seems to be preferable, but such a modification of it as the *lobus cavæ* is the best appellation, because it indicates the structure by which it is separated from the rest of the lung, and serves to distinguish it from another lobe occasionally seen, and which, from its relation to the vena azygos, is better entitled to the qualifying epithet of *azygos*.

Examination of thoraces in the mammalian series reveals the fact that the *lobus cavæ* is well represented in the majority of forms;<sup>4</sup> while in a few, of which man is an example, it is normally

<sup>1</sup> The mode of connection of the pericardium and thoracic parietes presents very different varieties in the different members of the mammalian series, being a purely pleuritic adhesion in some, and in others presenting delicate aponeurotic bands (ligaments) in addition; while in a few forms there is the further addition of an adhesion of the pericardium to a large or small area of either the diaphragm (*Simia*) or the costal wall (*Sus scrofa*), or both (*Delphinus*). For all these varieties of attachment, see Owen's *Comparative Anatomy*, vol. iii.

<sup>2</sup> The left phrenic nerve, after having passed the pericardium, lies between the two pleuræ. The nerve of the right side is within the fold which the right pleura makes round the vena cava.

<sup>3</sup> Owen uses both the first and second terms.—*Anatomy of Vertebrates*, vol. iii. pp. 576 *et seq.*

<sup>4</sup> The presence of the lobe in a well-developed condition is mentioned by Owen (*loc. cit.*) as occurring in the *Ornithorynchus*, wombat, kangaroo, potoroo, petaurus, phalangers, opossum, dasyurus, perameles, porcupine, pteropus, ai, rhinoceros, tapir, bears, ratel, wolverine. I have personally examined and found it well developed in the dog, cat, horse, sheep, ox, pig, peccary, lion, otter, fox, coati-mundi, guinea-pig, agouti, rat, rabbit, mouse, hedgehog, bat.



absent.<sup>1</sup> Between these extremes there is an intermediate group, in which an approach towards the formation of this lobe occurs.<sup>2</sup> In the mammals of this section the rudimentary pulmonic lobe is in the form of an angular process, which projects into a depression of pleura behind the vena cava.

It is in these prone mammals which have the deepest chests that the *lobus cavæ* is best developed.<sup>3</sup> The reason of this becomes obvious when we consider that length of thorax is to a certain extent dependent on its depth.<sup>4</sup> The axis of the heart in the prone position tends to assume the vertical direction, provided the thorax is sufficiently deep to accommodate it. In that case the short axis of the heart will correspond almost to the long axis of the thorax, and between the diaphragm and pericardium the lobe of the lung will find room in which to develop itself. It is a want of a combination of these conditions which renders the lobe small or obsolete. In the seal<sup>5</sup> the sterno-vertebral diameter of the thorax is not of depth sufficient to accommodate the long axis of the heart. The apex of the organ is therefore more inclined backwards towards the diaphragm than in most other mammals, and the *lobus cavæ* as a consequence is small.

In *Cetaceans*<sup>6</sup> the same relative shallowness of the chest occurs; but, in addition to this, the sternum is shortened so much, that the pericardium and diaphragm are brought into close contact, and become so extensively connected as to leave no space for the lobe, which is therefore absent in these mammals.<sup>7</sup>

<sup>1</sup> It is absent in Cetaceans, and is said to be represented only by a mere process in *Hylobates* (Owen); though Macalister (*Morphology of Vertebrate Animals*) states that the anthropoids have never got a "*lobus azygos*."

<sup>2</sup> In this group may be placed baboons and other catarrhines, in whom, according to Owen, the "*lobus impar*" is small.

<sup>3</sup> In estimating the length of the thorax, it is necessary to measure the cavity both dorsally and ventrally, for in Cetaceans the thoracic length along the vertebral column is extraordinary, whereas the length along the sternum is comparatively short, and, partly as a consequence of this shortness, no *lobus cavæ* exists.

<sup>4</sup> See footnote 1 on next page.

<sup>5</sup> Verified by personal observation.

<sup>6</sup> I have personally examined the thorax, and have seen the condition described in both the dolphin and the porpoise.

<sup>7</sup> The pericardium is extensively attached in these, not only to the diaphragm, but also to the sternum. May not this be due to the greater relative width of the heart which is characteristic of these mammals (Owen refers to this great width, *loc. cit.*), and which is probably associated with their aquatic habits?

In man, however, the conditions are the reverse of favourable to the development of the *lobus cavæ*; so much so that the lobe does not normally exist. The development of the lobe in mammalia is inversely proportionate to the amount of *pericardio-diaphragmatic* adhesion which occurs; when the adhesion is *nil*, the lobe is at its maximum; when the lobe is obsolete, the adhesion is extensive. The extensive *pericardio-diaphragmatic* adhesion which is so characteristically human is rendered so by the shortness of the chest and its narrowness from behind forward; and there can be no doubt but that it is on account of the action of gravity acting on the cardiac mass, from the human trunk being erect, that this adhesion amounts to a fusion of pericardium and diaphragmatic centre. The shortness of the sterno-vertebral diameter of the chest renders the assumption of the erect posture easy; it at the same time renders the cavity of the chest short:<sup>1</sup> it would, therefore, be a feature on which to place reliance in estimating in a mammal, from superficial observation of the thoracic parietes, the amount of adhesion (if any) which exists between the pericardium and diaphragm. External configuration of the chest is therefore correlated, not only with internal changes but also with the posture of the animal.<sup>2</sup>

The following are the cases of abnormal pulmonary lobes found in the human chest:—

I. An example of a *Lobus Cavæ* in Man.—During the course of an ordinary dissection of the thorax of an adult subject (Session 1879–80), my attention was directed to the part with a view of elucidating what was at the time rather puzzling, there being such a change from the normal condition. As the dissection had arrived at a rather advanced stage, I succeeded in preserving only a portion of the thoracic viscera, but took notes

<sup>1</sup> To explain what is so vaguely put in the text, it may be stated that if we could artificially produce the sterno-vertebral flattening, characteristic of the human chest, in one of the ordinary deep-chested brutes, the approximation of the sternum to the spine would (imagining the diaphragm to be stretched tightly in both positions), induce a movement of the muscular floor towards the thoracic cavity.

<sup>2</sup> I do not by this maintain that external form of chest is correlated with multiplication of lung lobes by fission, but with the peculiar variety of lobation of which this paper treats. Comparative anatomy teaches us that lungs may be altogether lobeless (Cetaceans), or may consist of lobes which are produced either by fission alone (man), or by constriction of vascular cords alone (elephant), or in both ways (majority of mammals).

on the other peculiarities, of which the following is a digest. The line of reflection between the diaphragmatic and pericardial parts of the right parietal pleura, instead of being as usual along the right edge of the caval opening of the diaphragm, was within the left margin of the vein. The vena cava formed therefore by its right side a prominent ridge, and was covered externally and posteriorly, before piercing the pericardium by the right pleural sac, which formed a fold round the vessel, and enclosed it along with the right phrenic nerve. The vessel, however, was lengthened only slightly beyond the normal. The right pleura projected behind the vein, and after covering part of the back of the pericardium, was reflected backwards to the right side of the œsophagus, forming the right boundary of the posterior mediastinum. The commencement of this layer, however, formed in the angle between the œsophagus, pericardium, and diaphragm a close connection with the pleura of the left side, the two membranes in that position lying back to back, as could be easily appreciated by embracing the sides of the pericardial bag with the hands and bringing the fingers of one hand towards those of the other behind it. This peculiar bi-laminar septum was placed more to the left than the right, and was occasioned by the prolonged depression of the right pleura, which passed behind the vena cava and pericardium. The depression formed a small *cul de sac*, the open mouth of which corresponded to the interval between the edge of the pleural fold containing the vena cava, anteriorly, and the *ligamentum latum pulmonis*, posteriorly, the serous projection being continuous with the anterior layer of the latter. The right lung gave off a small angular, blunt process, which passed into and accurately filled this hollow. This pulmonic process was distinctly marked off from the rest of the lung by a shallow, wide, vertical groove in front, which corresponded to the projecting vena cava, and was limited behind by the line of attachment of the *ligamentum latum pulmonis* to the lung. Inferiorly, the process was flat and continuous with the rest of the pulmonary base surface. When compared with the corresponding parts of the thorax of a domestic mammal, *e.g.*, rabbit, there is conclusive evidence of the identity of this process and the pleural depression which lodges it with the caval lobe and its sac in prone mammals. In addition

to this resemblance between lower mammalian forms and the parts displayed in this dissection, the preserved portions of the thoracic viscera, of which a preparation has been made, show that the affinity is still more complete. The pericardium is not firmly attached to the diaphragm, for they were easily stripped separate from one another; further, there is presented in the specimen a good example of a permanent left duct of Cuvier in the form of a previous left superior vena cava, of about crow-quill calibre, which, passing in front of the root of the left lung, receives posteriorly, from above the pulmonic root, the termination of the left superior intercostal vein.

A case having a bearing on this one, and probably belonging to the same category, is described by Professor Turner.<sup>1</sup> The chief peculiarities mentioned as existing in that case are:—The diaphragm and pericardium were unattached, the pericardium forming a perfectly free, movable bag; and as the vena cava proceeded upwards to pierce this bag it had a thoracic stage of about one inch in extent. As the lungs had been drawn on one side before Mr. Turner's attention was directed to the dissection, the relation of the right lung to the vena cava was not ascertained.<sup>2</sup>

II. *Azygos Lobe*.—Another abnormal lobe has been spoken of at the commencement as better deserving the name of *azygos* than that above described, for it bears the same relation to the *azygos* vein that the caval lobe bears to the inferior vena cava. Both lobes also belong to the same class, for, as will be seen afterwards, the vein and lobe in each case stand to one another in the relation of cause and effect. This lobe (*azygos*) has been noted twice in the pages of the *Journal of Anatomy*,<sup>3</sup> where also its nature was first described by Professor Cleland.<sup>4</sup> Since the Session 1877–78 up till the commencement of the present Session, three well-marked specimens of the lobe have been met with in

<sup>1</sup> "Case in which in man the pericardium was unattached to the diaphragm, with a parallel illustration from the walrus."—*Jour. of Anat. and Phys.*, vol. v. p. 141.

<sup>2</sup> The presence of a well-developed lobe of this sort in the human subject is recorded by S. Pozzi (*Revue d'Anthropologie*, 1872, p. 448). The lobe was of triangular shape, and about one-fourth the size of the middle lobe.

<sup>3</sup> Chiene in *Jour. of Anat. and Phys.*, vol. iv. (1870); also Cleland, *Ibid.*, vol. iv. p. 200.

<sup>4</sup> *Loc. cit.*

this medical school. In one case it was present in a foetus, in the others in the adult, and all were in every respect similar to those already put on record. Briefly the peculiarities in each are:—The arch of the vena azygos does not correspond to the upper border of the right pulmonic root, but is greatly elongated and lodged at the bottom of an oblique sulcus which indents the back of the apex of the lung and separates a bit off from the rest of the viscus. As the vein lies at the bottom of the abnormal groove, it is in the free edge of a reduplication of the parietal pleura, which fills the sulcus in such a way that the abnormal lobe lies in a sac whose open mouth is encircled by the azygos vein.<sup>1</sup>

Professor Cleland (*loc. cit.*) accounts for the formation of the azygos lobe, thus:—

“The great vena azygos, in its early development, passes upwards to open into the transversely situated right duct of Cuvier. By the descent of the heart from the cervical region into the thorax, the right duct of Cuvier becomes the vertically placed vena cava superior, and the great vena azygos is bent downwards till its terminal part becomes horizontal. What I believe then has taken place to produce the supernumerary lobe, is that there has been, at a very early period, a slight adhesion of the lung to the thoracic wall, or, much less probably, an undue curvature of the embryo, so that the vena azygos, as it bent downwards to a position at right angle to its original direction, instead of slipping behind the pleura and lung, dragged down a fold of the former, and deeply notched the latter.”

There seems to be no mention made in works on comparative anatomy of the presence of this lobe as a permanent condition in any mammal. In some, however, there may be seen an approach towards its formation. In the rabbit the vein has its arch placed very far forward (close to the first rib), and forms a prominent ridge, under which the mammillary superior angle (the part corresponding to the apex in man) of the posterior margin of

<sup>1</sup> E. W. Collins describes a lobe of this variety which he saw in a subject in the dissecting-room of Trinity College, Dublin. He also refers to other similar cases, and to an interesting one mentioned by Wisberg, where the lobe was from the left lung, and in connection with the superior intercostal vein of that side, indicating the possibility of a lobe being cut off by either of the Cuvierian ducts as they alter their position.—*Trans. Roy. Irish Acad.*, April 1874.

the lung fits. In fact, the relation of the parts is such that a backward movement of the venous arch would produce an azygos lobe. In a porpoise, dissected here lately, the lobe is present in a largely developed condition; the fold of the pleura has, in fact, divided the elongated apex of the right lung into two equal parts, the fissure reaching down almost to the level of the lung root. That this state of matters in the porpoise, is the rule or the exception is, however, problematical, and requires an examination of more than a single specimen to determine.

III. *An Azygos and Caval Lobe existing together.*—One of the adult subjects referred to under heading II. presented this double abnormality. The subject, a male, was dissected here last winter session. The azygos lobe was larger than in the other cases, being made up of the whole of the apex of the right lung, and the sulcus which cut it off was horizontal, and not oblique. In other respects it was like the others. In addition to this azygos lobe there was a caval lobe also present. Like the other abnormal caval lobes, it was simply a lip of pulmonary tissue, which projected behind the vena cava, the vessel being covered on its right side by pleura, on account of the area of adhesion between the pericardium and diaphragm being smaller than usual. In this case also a peculiar fibrous band (ligament) existed, which is interesting as being the representative of some of the pericardial ligaments of prone mammals. The band which was flat was three inches long, and about a quarter of an inch broad, and connected the front of the pericardium with the costal cartilage belonging to the seventh rib of the left side.

In proceeding to investigate the mode of formation of these lobes, it is at once apparent that the vein lying in the groove, which circumscribes or limits the lobe, was instrumental in the production of that lobe. In this peculiar process of lobe formation, the principal requirement is that the vein should be of sufficient length, and that through it a resistance should be applied sufficient to obstruct the development of the lung mass. In the case of the azygos vein the force was represented by the movement of the heart from the cervical region, which was sufficient to make the vessel cut into any lung tissue, which through accidental circumstances came in the way. The force which acts on the lung tissue through the vena cava in the

production of a caval lobe, is probably also resident in the cardiac mass. By a movement of the heart on its long axis the vena cava inferior passes from its original central position towards the right, and, in those mammals in which its intra-thoracic length is great, presses against the lung, and produces a caval lobe, which is increased in size by the fact that at the same time that the vena cava moves to the right, the mediastinum is being pushed to the left and the right lung grows towards it. This same growth of the parenchyma of the lung after the receding right mediastinal layer of pleura, is further evidenced in some prone mammals by the projection of a large tongue-like process from the sternal edge of the right lung across the middle line. The process lies in front of the pericardium, and overlaps part of the origins of the great vessels and thymus gland, and is in some mammals, *e.g.*, the ox, so large as to merit the title of lobe, although, unlike the azygos and caval lobe, no well marked constriction defines its origin.<sup>1</sup>

The retrogression of the right mediastinal layer, on which the presence of this lobe is dependent, is probably associated with the atrophy of the lower part of the right aortic arch, and a want of development of other parts of the right branchial arches. In man, the chest flattening does not admit of the formation of this lobe, but occasionally its representative may be recognised as a thin process which projects from the anterior margin of the right lung, and which in inspiration crossed the middle line about the thymic region. Such a process when large, and its better developed homologue in Ruminantia, should be called the thymic or aortic lobe, so as to distinguish it from the other abnormal pulmonary lobes in man, or from these which in comparative anatomy occur normally.

Seeing that the form of chest flattening characteristic of man is unfavourable to the development of these lobes, can we by embryology account for the different forms of chest flattenings? At the time that the heart descended from the cervical region the chest walls which surrounded it were of the same shape in all mammalian forms. But by the pressure of other parts of the

<sup>1</sup> It is known as the "anterior lobe of the right lung" by comparative anatomists, and is best developed in Ruminants.—See *Chauveau*, by Fleming, p. 470.

foetal mass on this barrel-shaped thorax, a change of shape is brought about, for the whole foetus is being as it were moulded to the shape of the chamber which holds it. The moulded mass assumes the cylindrical form in bicornuate uteri, and the ovoid in the pyriform human uterus. The large size of the human foetal head and its bent position on the front of the chest, where the arms are already folded, causes the chest to be pressed on anteriorly; hence a sterno-vertebral flattening is the result. In other mammals, however, the gradually thickening limb arches will, in preserving the cylindrical form of the foetal body, grow at the expense of the rotundity of the body cavity, producing through the arches of the fore limbs the bilateral flattening of the thorax, which in prone mammals remains unchanged throughout their whole after development.



# CRITICAL REMARKS ON POLYDACTYLY AS ATAVISM.

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INSTANCES of supernumerary fingers and toes have been frequently recorded and minutely described, but great diversity of opinion exists as to their explanation. It has been held by some that they are simple monstrosities, whilst others have assigned to them a deeper meaning, and, looking on them from an evolutionary point of view, consider them to be a reappearance of a more primitive organisation, or a reversion (Rückschlag) to a primary state,—in a word, an atavism.

At first sight, the second view would seem to be the more satisfactory, since many cases can be explained by it, which if simply looked upon as mere malformations, would be disconnected like other "abnormalities." The first view appears to be the more simple, as it does not suggest other conditions or require speculation. Considering, however, that the idea of "abnormality" does not involve anything positive, it is doubtful whether the former view has any advantage over the latter, satisfactory evidence against which can only be obtained from evolution.

Cases of polydactylism may be arranged under two categories, according to the number of digits normally present in those animals where it occurs. Assuming that the normal number of fingers and toes in mammals is not more than five, polydactyly in those which possess that number is of a different kind from that occurring in those which normally have less than five, even though the polydactylism should complete the number to five.

The first variety occurs chiefly in man, at least it is most frequently observed in him. In it, the number of abnormal supernumerary fingers and toes may vary till they equal that of the normal digits, the extra ones being developed on either margin of the hand or foot. Those cases, Rüdinger has stated, must be considered as reduplications, and therefore as malformations.<sup>2</sup> "This mode of looking at them," he says, "certainly suggests itself more readily, and is easier to prove, than Darwin's supposition that polydactylism is an atavism." Rüdinger has tried to show the correctness of his idea by throwing doubt on the accuracy of Darwin's statements regarding the reproduction of amputated supernumerary fingers. Darwin considers the presence of these fingers to be a manifestation of a lower, and to a certain extent an embryonic, condition, while he regards their reproduction as a process of budding similar to what is found in the limbs of amphibia. If the facts collected by Darwin are correct, as to which he himself of course

<sup>1</sup> *Morpholog. Jahrbuch*, Bd. vi. s. 584-596.

<sup>2</sup> *Beiträge z. Anat. d. Gehörorgans, etc.*, München, 1876.

was unable to judge, certainly the suggestion based upon them is not to be discarded. Rüdinger, on the other hand, gives a large number of cases, communicated to him by German surgeons, in which there was no regeneration of the supernumerary fingers after they had been once amputated, and from this he thinks that he has been able to disprove the atavistic meaning of those formations. This would be the case, indeed, if Darwin had based his explanation *exclusively* on the existence of such regeneration, and if the idea of an atavistic organ necessarily involved power of reproduction, a condition which it has never occurred to any one to suppose. Fingers and toes can be of an atavistic nature equally as well as other organs without showing any trace of reproductive power, in case of loss. Rüdinger's argument therefore appears to be inapplicable. Again, he is mistaken if he believes that we gain more than a simple name by calling such cases "malformation" or "abnormality," because atavism itself is an "abnormality," and "malformation" explains nothing; neither does the statement that this or that explanation suggests itself more readily prove anything. We must therefore seek for entirely different reasons than those used by him.

In the first place, we have to keep in mind the atavus to which this augmented number of fingers is to be referred. Then it must be proved whether the conditions under which the reversion (Rückschlag) occurs, can be referred to the atavus. As regards the polydactylous atavus itself, Darwin has already conceived it as existing at an early period in the evolution of vertebrates. Indeed, those atavistic forms of the limbs cannot be found in existing reptilia or amphibia, but must be sought for far below them, amongst the fishes or such fossil reptiles as the *Enaliosauri*. Of fishes, only the *Selachia*, *Chimæra* or *Ceratodus* need be considered, because there exists only in their limbs a greater number than five such skeletal elements, as become transformed into fingers and toes from the amphibia upwards. Of the *Enaliosauri* only the *Ichthyosauri* were polydactylous. The enormous distance from those fishes up to the mammalia, makes it scarcely probable that the polydactylism of the latter can have had its origin among the former. The distance is so great, that even the skeletal parts of the limbs which have been preserved by inheritance have undergone such extraordinary transformations, that their homologies are misinterpreted and still not universally acknowledged. The *Ichthyosauri* apparently approach somewhat nearer to the mammalia; they are, however, differentiated in a special direction divergent from that of the mammalian stem. If, then, we accept polydactylous forms as the ancestors of the mammalia, the limbs of those animals must have been very different from mammalian limbs.

In the second place, we have to consider the conditions under which polydactylism appears. It must always be remembered that *supernumerary parts are both in their external appearance and structurally, a repetition of the digital type which is expressed normally at the ends of the limbs as fingers and toes* [are reduplications of fingers and toes]. In cases where a lower and older stage reappears by atavistic force, one would expect that the product of such would exhibit ancestral

characters. Polydactylism of the lower vertebrates, as far as we know, does not consist in the fact, that instead of there being five fingers, as in man, there is a greater number of similarly shaped digits on a hand, but is a factor which influences all the skeletal parts of the limbs, and therefore intimately connected with the condition of the whole organism. Consequently, it is as impossible to conceive of polydactyle limbs being transformed into pentadactyle ones, unless we admit that important changes have occurred in the condition of the whole limb, as it is to imagine the sudden appearance of a part which had previously existed in an old polydactyle form, without (according to the laws of correlation) the other parts of the limbs being influenced, and being atavistically affected in some way or other. Otherwise we must suppose that the momentum of inheritance, after having remained dormant for an indefinitely long period, on reviving reproduces only the type, and that each specialisation can be adapted to that state which in the meantime has been acquired by the whole organism. The type idea, then, would consist in the presence of a series of skeletal parts, which by secondary adaptation become specialised into phalanges, metacarpals, and carpals. This specialisation would have originated under the influence of the higher organism in which the atavism appeared. Whether this view is correct or not may be doubted, but it is certain that adaptation of atavistic parts to the rest of the limb does not take place in such a way that the product represents a lower state of development. Supernumerary fingers and toes, we find, may be attached to the inner or outer margin of the hand or foot in every imaginable way, and may vary in number. They may also occur in the digital series itself, by the longitudinal division of individual fingers. In cases of polydactylism, where no digital division occurs, or where the supernumerary finger is only a rudimentary appendage, it is not uncommon to find that the bones of the carpus and tarsus are increased in number. The arrangement of these supernumerary bones never, however, indicates a lower state, not even in those instances where they do not possess specific forms. Great weight must be attached to these circumstances, as they illustrate the two following important facts:—Firstly, they show that the new apparition (*Erscheinung*) *has no connection with any definite lower form*. If any connection did exist, the atavistic parts could not appear promiscuously. Reappearances of an earlier form can only occur at the spots corresponding to those at which they existed in the atavus. These facts do not support the migration theory of atavism, but, on the contrary, show its weakness. Secondly, they show that increase in the number of component parts in those polydactyle forms cannot be traced beyond the carpus or tarsus. If continued further upwards, which has not hitherto been observed in man, but has been found in animals, it would result in a complete reduplication of the limb, and so of course could no longer be considered atavistic. We have, therefore, only to deal with increase in the terminal parts of the limbs as far as the carpus and tarsus. It has been elsewhere shown that the skeletal parts of the extremities originate from a series of rays attached to the limb-stems (*Gliedmassenstämmen*), the

single segments result from the splitting up longitudinally of these limb-stems, while the terminal series represent the bones of the digits.<sup>1</sup> Multiplication of the parts which correspond to the fingers, namely, the rays, seems to be based upon a multiplication, not only of the terminal, but also of the more proximal parts of the limbs. Consequently, the anti-brachial or the crural segment, and the humeral or the femoral segment should also, as will be well known to every one acquainted with the theory of the vertebrate limbs already quoted, participate in the multiplication, but this is not the case. It would be very remarkable if, in every instance of polydactylism, only the distal part—a segment of a ray—be preserved, whilst the proximal part, which is for binding the distal parts with the limb-stem, or girdle, is invariably lost.

We see therefore, on examining more closely, on the one hand, the conditions supposed to be atavistic, and, on the other hand, the structure of normally polydactyle limbs, that no bases can be obtained for the establishment of that supposition [that the parts are atavistic]. Neither the condition of the single parts in polydactylism, nor their mutual relation, support the atavistic theory. There is nothing indicative of a lower organisation except the increase in number for which atavism would be the only possible cause, if we accept that it is the only thing which is capable of producing such an increase. But this is not the case, since it is quite as likely to be due to reduplication, with which we have to reckon, quite as much as with atavism. If therefore the existence of polydactylism cannot be proved, in limbs already pentadactyle, we must abandon the hypothesis [of atavism].

The other form of polydactylism previously mentioned is found in those mammals which have normally fewer than five digits on each limb. They are derived from pentadactyle forms. If the total number of fingers, including the supernumerary ones, exceeds five, the case has to be regarded as identical with those already considered; but if, however, the total number is less than five, there are other conditions [which require to be considered], though here the explanation of the supernumerary fingers as being atavistic formations is also attended with difficulty. Firstly, the atavus would be less distant, because it must have belonged to the same class, since in this pentadactylism has already become typical; secondly, there still exist, in many cases, remnants of aborted fingers or toes, to the importance of which for redevelopment into complete digits Darwin has already directed attention. Thus we see here two extraordinarily favourable opportunities for the development of polydactylism from atavism. We must, however, examine these cases more closely. Polydactylism, as is well known, is most frequently found in swine, and chiefly in their fore-limb. In course of time many such cases have been examined by us, and though the details of each has been forgotten, the general results agree exactly with those of a case recently

<sup>1</sup> For a fuller account of what is here referred to, see *Morph. Jahrbuch*, Bd. ii. p. 396.

examined, which was received from a very competent source as "a thoroughly genuine case of atavism." It consisted of the four limbs of which the hands unfortunately were cut off at the carpo-metacarpal joints, so that no part of the carpus was preserved, and only a few of the metacarpals had their proximal ends remaining. What did remain, however, was sufficient for comparative examination. On both hands (manus) the 2-5 fingers were quite normally developed, but on the inner or mesial side of the second digits of each manus there was, in addition, a first digit, so that the pentadactylism was complete. It was remarkable that digits III. and IV. were of equal size, so that there was no gradation in the length of the fingers from the medius outwards, as in the normal pentadactyle hand. The digits II. and IV. were likewise of equal size. The artiodactyle shape of each manus seemed therefore complete, only with the addition of a first digit, the atavistic production. An examination of the bones show that in the right hand there were five metacarpals, the articular surfaces of which were normally developed. Of these metacarpal III. was the most important. It was broader than usual, with a large articular facet on the upper surface of its proximal end, for articulation with the third carpal, and two lateral facets for the second and fourth carpals. From the important investigations of W. Rowalevsky in ungulates, it appears that this condition of the proximal ends of the third metacarpal is met with, for the first time, in recent forms of pigs. In the fossil remains of the *Suina* of the Miocene period, this broadening of the bone towards the radial side does not exist. In *Cherotherium* metacarpal II. articulates as usual with the second carpal, but also with the third carpal on its radial aspect. In *Palaeocherus* metacarpal II. is separated from the third carpal, metacarpal III., however, not only articulates with it, but extends to the second carpal. In *Sus* the second carpal has two facets of nearly equal size, of which the one on the ulnar side articulates with the superior and internal processes of the flattened end of metacarpal III., whilst the other articulates with metacarpal II. This arrangement shows that the carpal articulation of metacarpal II. has a tendency to diminish in size. Finally, in *Dicotylus* the second carpal articulates only with metacarpal III., whilst metacarpal II. has entirely lost its articulation with the trapezoid. This shows that the carpo-metacarpal articulation, which was characteristic of the ancestral *Suina*, has been lost in the genus *Sus*.<sup>1</sup> One would expect that if an atavistic form of hand occurs in this latter genus, there would also be some trace of those conditions just described. The correlations of the different parts justifies this expectation. By the addition of a new finger the whole hand is changed, so that it can no longer be called artiodactyle, and one might expect other lower conditions besides this finger to appear; in a word, that artiodactylism had disappeared also in the carpus. In the present case, and in many others which we have examined, there was no indication of this, and everything pointed to a recent form.

<sup>1</sup> "Osteology of Hyopotamidae," *Philos. Trans. Roy. Soc., London*, 1873, p. 76.

Since these considerations give little support to an atavistic explanation, let us consider digit I, the one in which the supposed atavism appears. This atavism, however, is no longer present in ungulates, since pentadactylism has not been recognised in any of them. While the metacarpus (and also the carpus) indicated a recent form, the presence of a first digit gave the hand an appearance still older than that of even the Miocene *Suinae*. It could not be made out whether metacarpal I, which was a little smaller than metacarpal II, articulated with the first carpal. If it did so, the otherwise rudimentary first carpal would most probably have been larger. Metacarpal I was somewhat shorter and feebler than metacarpal II, and its digit was still more so; but of most importance is the fact, that this first finger had *three* phalanges, of these the first, or proximal, was longer than the other two together, the second was short and thick, and the terminal one, though more slender, was hardly longer than the second. From this it follows that the first finger could not represent the pollex which, in mammals, except in the *Cetacea*, has always only two phalanges, it being exactly similar in all respects to the other four fingers. The idea of atavism in this finger must consequently be abandoned, since the unknown pentadactyle ancestor of the *Ungulata* could have only possessed a bi-phalangeal pollex on this part of the hand, like every other pentadactyle mammal. This fact must be kept once for all clearly in mind. Closer examination, therefore, shows that we have absolute proof that there is nothing in the one manus which can be considered atavistic.

The external appearance of the left manus was similar to that of the right; but there were only four metacarpals, that of the first digit being absent; its place, however, was taken by metacarpal II, which had its distal end flattened out, and carried the second and first fingers. The phalanges of the first finger were somewhat larger than the corresponding ones of the other hand, and were of the same size as those of the second finger. As this first digit had likewise three phalanges, it cannot be said that we had here such an atavistic state in which the first finger was disappearing by suppression of its metacarpal or by amalgamation of that bone with the second. Moreover, a metacarpal does not disappear by transferring its function to an adjacent one, as can easily be proved in mammals. A finger does not become suppressed before it has lost its function, and its metacarpal bone is not affected before this has taken place. The following circumstance will show how careful one must be in explaining such cases. As the additional finger has three phalanges, we have come to the conclusion that it cannot be an atavistic formation, but must be an example of reduplication. This conclusion cannot be reversed. We cannot say that the existence of a smaller number of phalanges is a sign of atavism, because it is quite possible that the accessory finger, notwithstanding its smaller number of phalanges, is *not* a pollex; such additional parts being often incomplete. Ontogeny likewise does not favour the atavistic theory, since no germ of the first finger has hitherto been found in any embryo. A. Rosenberg, who looked for this in vain, thinks that "perhaps metacarpal I may be still traced,

since it, as must be inferred by analogy, would have disappeared at a later period than metatarsal I."<sup>1</sup> Thus only the first carpal remains, and that "in a somewhat less differentiated condition than the other carpal bones." This ontogenetic condition is phylogenetically accompanied by the very early loss of the first digit, together with its metacarpal.

To sum up, we have proved that the case just described, like others so frequently found in pigs, cannot be considered as atavistic. We have here to deal with a monstrosity, which must be placed to the category of reduplication, and which in the instance described varied in form in each manus, since in one manus it was traceable proximad as far as the metacarpal bone, whilst in the other it affected the whole metacarpal. On account of the product of this duplicity being *one* finger, the manus had a pentadactyle appearance, and the first finger would have been considered a pollex, but for its three phalanges.

This supposed case of atavism has been so fully considered in all its bearings, in order to indicate the points which appear to be of importance for critical examination of similar cases. The atavistic theory itself does not lose in weight by the elimination of cases which do not belong to it. It is only made clearer thereby, and gains in importance when the range in which the atavistic appearances can occur is restricted by the exclusion of those conditions which indicate a reversion (Rückschlag) to a very early form. It must also be acknowledged that the occurrence of atavism in cases of complete neomorphism (Neubildung) is contradictory to the theory of descendancy, since it breaks the continuity of inheritance. Atavism can be admitted only in those cases where parts normally rudimentary are developed from a still existing germ.

In proportion as the position of the atavistic model is less remote, so are the difficulties of understanding its appearance diminished. Thus it is comparatively easy to explain the existence of a supernumerary digit (IV.) in the horse, since it can be traced back to comparatively recent tridactyle ancestors.<sup>2</sup> Hensel's investigations show it to be probable that the finger typically attached in *Hipparion* to the medial styloid bone becomes last aborted. Consequently, when reappearance of an ancestral form occurs in the horse, it is this finger which is reproduced. This accords with a very interesting case, lately described by Prof. Marsh,<sup>3</sup> in which a second digit was completely developed on both the anterior and posterior limbs of a horse. We see, therefore, that those cases occurring in horses (first rightly propounded as atavism by Hensel), do not belong to forms palæontologically remote and systematically far distant. It is not the ungulate group of the Perissodactyles alone which are included, but even the family [?]. The question might arise, whether in such cases of atavism, skeletal parts of supernumerary digits are not perhaps a further development of germs usually latent. In the horse, A. Rosenberg was unable to prove

<sup>1</sup> *Zeitschrift f. Wissensch. Zoologie*, Bd. xxiii. p. 122.

<sup>2</sup> Hensel, "Ueber *Hipparion* Mediterraneum," *Abh. k. Akad. d. Wissensch. Z.*, Berlin, 1861, p. 66.

<sup>3</sup> *American Journal of Science and Art*, vol. xvii. June 1879.

this; however, the question does not seem to be finally settled, as more material is required than was at Rosenberg's disposal.<sup>1</sup> In any case skeletal parts which appear in the earliest differentiation of the embryo, and become suppressed later on, do not play an unimportant rôle. Atavism consists not in the existence of a latent germ, but in its becoming perfected and further developed. In this lies the reversion (Rückschlag). We have in the next place to distinguish between two different forms of atavism. In the one the atavistic part exists by the law of inheritance in the early embryo as a germ which normally disappears, but which in some cases becomes further developed. An example of this form is seen in the *os centralia carpi* of man. In the other form the atavistic part is not found normally in the germ, because in it the reversion has already taken place. Examples of this form are afforded by the horses mentioned above. The first form we propose to call "Palæogenetic," the second "Neogenetic" atavism.

Before closing this discussion, there is a point already referred to which requires further consideration, namely, the idea of malformation or monstrosity, under which category of morphological conditions the case of the pig already discussed has been classed. With improved methods of research, these conditions are better understood, and we have no longer any right to regard them as "preter naturam," since we now know that quite analogous conditions occur normally. We are accustomed to explain malformations "per defectum" by ontogeny, as an arrest of development at a more or less early stage, and an attempt has been made to explain malformations "per excessum," so-called double monsters, by tracing them back to conditions usually found in lower vertebrates.<sup>2</sup> The polydactylism of mammals belongs to the latter category; but it is not, however, so widely different from the first as it would at first sight seem to be. The ontogenetic stations have long been known to be persistent in lower forms; those appear again in the ontogeny of the higher forms, since they existed in their ancestors. Thus ontogeny in such cases is only a transitional stage between malformations and phylogeny. We might, therefore, speak of atavism in malformations. This is not done, however, since in them another condition appears, namely, want of adaptation. Cleft palate in a lizard or bird is normal, and the other organs are correlated in structure and function to that condition. Cleft palate in a mammal is an abnormality, as the other parts are not in correlation with it. Malformation is, therefore, a pathological condition, and must be distinguished from atavism, with which, however, it is connected through the neogenetic forms of the latter. The difference between them, however, may not be great, as want of adaptation, or, in other words, malformation, varies in degree. In any case, to regard certain conditions as malformations, and even to constitute such a category, does not mean to doubt the soundness of the organic world; on the contrary, a careful study of what malformation is, only makes its morphological position clearer.

<sup>1</sup> Rosenberg, *Morpholog. Jahrbuch*, Bd. i.

<sup>2</sup> Rauber, *Morpholog. Jahrbuch*.



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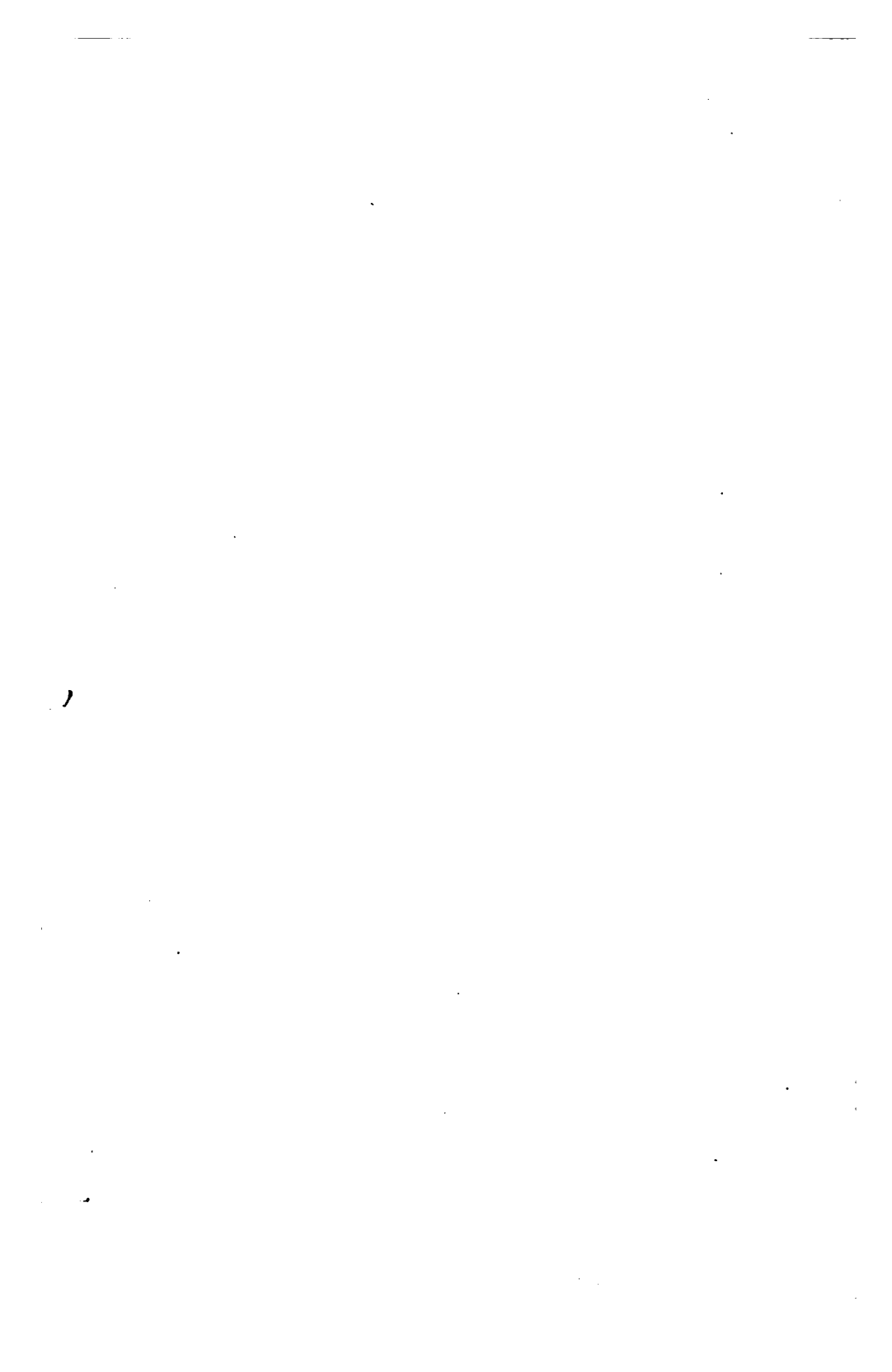
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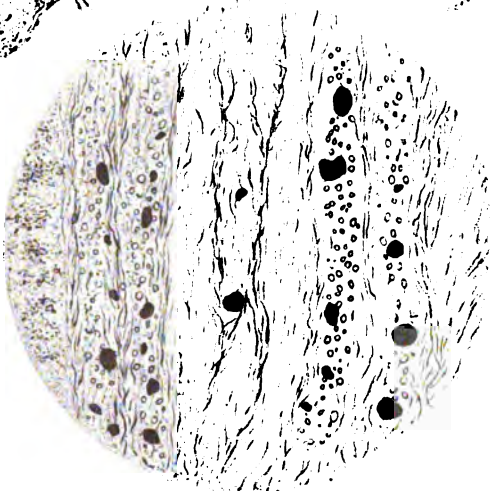
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